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THE PSYCHOLOGICAL REVIEW

INNATE CONSTITUENTS OF COMPLEX RESPONSES IN PRIMATES

BY PAUL H. SCHILLER [1]

Yerkes Laboratories of Primate Biology, Inc.

Adaptive behavior is recognized as constituted of unlearned patterns, *modified* by repetition or reinforcement. What the original responses are like, however, before they are modified by learning, is not frequently analyzed in the literature. Yet how can modifications of behavior be studied if we do not have a basis of comparison in the *unmodified* patterns analyzed as extensively as the learning series? Some comparative psychologists do not seem to study the animal before training; consequently they are inclined to hold that all adaptive responses are learned and do not conceive of any complex response as native.

As a matter of fact, many a conditioned response has been found to be paralleled by unconditioned responses that appear when the animal becomes adapted to an originally ineffective stimulus. The organism has a changeable reserve of responses, influenced by general and specific levels of activity. This level varies with the internal state, the motivation and adaptation of the organism, as well as with the frequency and consequences of a particular behavior pattern. Since we cannot control all internal stimuli, we had better start with comparative studies of early behavior and make a complete inventory of the animal's repertoire.

Just which response is complex and which simple is not easily decided in the light of embryological behavior studies. Elements of complex responses are admitted to be ready prior to specific learning, but the question is whether the organism is not producing the compounds we observe without any training, just as its effectors mature. Experiments on maturational factors *versus* experience have led to contrasting results with various species and various tasks. In a long forgotten study Spalding (39) has shown that flying in birds was unimpeded after he prevented early practice. Essentially the same was found by Carmichael (5) in the swimming of tadpoles, by Gesell (8) in the climbing behavior of human twins. On the other hand, Shepard and Breed (36) recorded that chicks have to learn how to peck seed in a few days of practice, whether freshly hatched or fed artifically for a considerable period allowing for maturation. Similarly Stone (40) and later Beach (2) have found severe impairment of copulating behavior in rats isolated from early contact with mates.

The contradiction in these results can be resolved by a dichotomy. The *constituents* of the motor pattern themselves mature. Due to internal, prefunctional factors, they appear at a cer-

[1] This paper was prepared at the Yerkes Laboratories of Primate Biology, and at the editor's request was revised and condensed, after the author's death, by Professor Lashley.

tain stage of development (many of them traced in embryos) ready formed. Their *application* to external stimulus configurations is something that must be learned. Such a dichotomy was reported by Moseley (24) who found that pecking, striking and swallowing were unlearned responses whereas seizing of the grain was formed by practice.

In his comparative studies on birds, Lorenz (21) arrives at a substantial distinction of the variable and non-variable parts of "instinctive" behavior. He regards the latter as the instinctive component proper and identifies it with hereditarily coordinated, centrally steered "automatisms," of which the fin movements of fish, flying of birds, or the periodic thrusts of copulation in mammals are examples. These patterns rarely appear in clear-cut isolated form, for they are interlocked with orientational reactions that guide the effectors to the proper place where they can carry out the automatisms with effect. Shaking the bill that holds a twig will occur quite frequently in some birds, but only if it is oriented to a pile of interlaced fibers where its end will be caught in a nest being constructed. The retrieving of the egg that rolled out of the nest mound of a goose is initiated by a thigmotactic balancing of the chin on the top of the egg, with outstretched neck, whereafter the neck is bent and lifted in the sagittal plane to drag in the egg, and its rolling aside is prevented by corrections of its position relative to the ventral portion of the beak. The thigmotactic component of this behavior compound can be elicited practically always, by presenting a white object not far from the nesting bird, but the sagittal movement of retrieving is exhausted readily after having been displayed a few times. Orientation is stimulus-seeking and produces the ultimate elicitor; it is the appetent component of adaptive behavior that initiates the consummatory phase, the innate coordinated automatism.

Following Craig (7), Lorenz conceives of appetent behavior as consisting of modifiable orientational reactions due to the influence of experience. The autonomous motor pattern itself, however, is emitted without any variation in form, once the proper contact with the environment is established. The latter is the hereditary component of adjustment, depending in frequency on changeable response levels according to some specific organic state of need. The specific reserve for such motor patterns can be individually accumulated or exhausted, whereas the orientational responses can be elicited at any time and decay only with the general fatigue of the organism. Lorenz, to be sure, does not restrict the hereditary factor exclusively to motor patterns, but thinks perceptual "innate schemata" responsible for their release. The intricate decoration of some birds is said to serve for sexual selection without any specific learning. The perceptual pattern releases a trigger mechanism that liberates the centrally steered automatism.

It is not necessary, however, to presuppose such an innate connection of these discriminanda with mating responses. It is quite possible that the animal learns by trial which of his partners respond correspondingly to his sexual approach and which do not. Conradi (6) reported long ago that sparrows reared among canaries display social, even singing, patterns very similar to the foster family, but adjust to their own kin readily after being reunited with sparrows. Lorenz himself describes the broad generalization along which young birds will follow comrades: Initial response will be given to practically anything that moves and is of a certain order of magnitude. All approachable, graspable, etc. things release the motor patterns which fit or

are appropriate to them. What perceptual properties will in future be worthwhile reaching for, is decided by gradual differentiation on the basis of reinforcement and extinction, i.e., the animal learns to produce by his motor response more specifically conforming perceptual entities.

There are, of course, certain limits to the original form-equivalences as well as to their change with varying needs, habituation, or association. A range of size in acceptable substitutes is limited not only by perceptual thresholds, but also by the order of magnitude in the effectors that can make use of the perception. Such a conformity in the temporo-spatial qualities of sensory and motor behavior fields determines correlations without any specific innate connections. The plasticity of the structural equipment of an organism can only be determined by exploring the extreme modifications produced in releasers by early fixation and by training.

There can be no doubt that adaptive behavior is shown in typical "instinctive" patterns. But similarly rapid and definite adaptive modifications occur in some cases of complex learning too, in problem-solving behavior, described as "insightful."

The animal seems to have an understanding of the actual situation and behaves as if steered by orientational cues. In studies on the detour behavior of various animals I have arrived at a composite notion of behavior adjustment rather similar to the cooperation of internal and external factors derived from the aforementioned investigations on instinctive constituents. Direct attack and giving up (fish, **30, 31**), exploratory responses and nest-building incipients (rats, **1, 32**), seeking and covering (octopus, **33**) constitute the complex plan of the detour pathway chosen in every new situation. A familiarization process extinguishes the repetition of local reactions and lowers the threshold for orientational stimuli. After repeated exposure to a frustrating situation, the originally independent reactions are integrated in time. Strictly speaking, the animals do not learn to make a detour. They make it in reference to the presently governing stimulus conditions produced by their own behavior. It is the consistency of external events, their lawful sequence and intrinsic relations that secure a consistency of organismic responses to them. The performance actually observed is a change of speed in the serial elicitation of previously determined response patterns.

Since the clearly innately determined activities and the rather complex learning processes thus appear to follow identical laws in orienting the animal in its adjustment, it seems worthwhile to summarize some as yet unpublished data of mine under this viewpoint. Four fields of experimentation illustrate the point: (1) discrimination and (2) conditioning of presumably emotional perceptions in apes, (3) solving problems with the use of sticks, and (4) spontaneous stick-using in apes and monkeys. They show, as I believe, that the motor constituents are innately determined, but their organization can be stabilized in more or less comprehensive units on the basis of reinforcement. Perceptual guidance is originally restricted to dimensional steering rather than to the configurational properties that become functional with learning.

Do Certain Visual Configurations Elicit Unlearned Motor Responses?

Although on the level of orientational responses there seem to be, if we allow for broad equivalences, predetermined sensorimotor correlations, like eyeblinking and startle, this type of inherited reflex cannot be simply extended to more complex patterns. What specific

perceptual organization will elicit any given motor integration is decided in the course of functional activity. Certain processes in the repertoire of structural capacity will be selected to constitute a pattern, more or less permanently fixed. Situational factors integrate motor activities to adjust to the environment.

Experiments on animals reared in the dark and maturing without visual experience have demonstrated, after release, some remarkable orientational achievements. Lashley and Russell (20) found that rats would jump, under such circumstances, with a force proportionate to the distance of visual objects. Hebb (13) found an immediate preference for darker regions, for moving and outstanding parts of the visual field, and no impairment of transposition learning. Riesen (28) reported on infant chimpanzees who displayed blinking and visual pursuit after prolonged confinement to a dark room, but gave no responses to complex stimulus configurations, to discriminate which took weeks of practice, and, to utilize properly, several months. The duration of functional loss seems to be roughly proportionate to the length of the confinement. The contact with the visual environment certainly needs visual experience, especially in an animal normally steered chiefly by visual cues: After having been forced to use other sensory modalities, these poor cues can only gradually be replaced by the sudden release of a more appropriate modality of signs. This holds even for a young animal that had ample visual experience before he was placed in the dark room. These experiments certainly did not reveal any "recognition" of perceptual configurations "known" instinctively. And this cannot even be expected unless the notion of innate schemata is taken literally. While a certain conformity of the temporo-spatial (dimensional) characteristics of perceptual objects and performable motor patterns (location, size, intensity, persistence) seems to be secured by the native endowment of capacities, and to work without fixed connections in guiding adjustment, there is no evidence of any specific motor relation to complex perceptual configurations in higher animals.

A number of students have described innate recognition of members of the same species (16, 18, 34, 35, 42, 43) but their results are open to criticism (26) and are contradicted by other studies (21, 42).

Although the young animal may fail to give an immediate differential response to a given object, some innate tendency to response may be revealed by differential learning rates for different objects.

Almost all adult chimpanzees show marked fear of snakes (12, 14). This is not true of infants, and the fear is more persistent in older adults than in youngsters (12). The origin of this fear is not known but, if it has an instinctive element, this should be revealed by differences in the rate of formation of approach and avoiding reactions to snakes and to other visual objects which never normally elicit such fears. Two experiments were carried out to test such differential learning.

1. Two infant chimpanzees about two years of age were used in this experiment. They had been kept in darkness for the first year or more of life and thereafter in separate cages, so that their experiences were extremely limited. They were trained in discrimination between a whole orange and a dead, coiled coral snake, each in a glass jar. Neither animal had ever seen a whole orange (they are fed small pieces) or a snake. They were trained to touch the positive jar, with reward of milk from a bottle, and to avoid the

TABLE 1

NUMBER OF TRIALS AND ERRORS NECESSARY TO MAKE A RELIABLE DISCRIMINATION. THE ROMAN NUMERALS SHOW THE ORDER OF EXPERIMENTS

Chimpanzee	Snake+ Trials	Orange− Errors	Snake− Trials	Orange+ Errors
S.	I. 732	247	II. 706	278
A.	II. 323	132	I. 255	93
A.	IV. 243	85	III. 302	127
Average	433	155	421	166

negative with electric shock or later a gentle push as punishment. Training was continued to 45 of 50 trials correct. For animal S the snake was first positive; for animal A, the orange. Training was then reversed (Table 1, experiment II) and for animal A reversed a second and third time (experiments III and IV). As shown in Table 1, there is no significant difference in the rates of acquisition of reactions to objects which do, or do not at a later age call out marked fear reactions.

2. A second experiment was devised to test whether the fear reaction, once established, by maturation or learning, is more difficult to change by conditioning than is a positive or indifferent reaction. Fifty-two chimpanzees, ranging from 1 year to 30 years of age, were stimulated repeatedly by the presentation of live, moving snakes confined in a glass box. Thirty-four animals consistently displayed avoidance reactions, with definite inhibition of locomotion, specific vocalization, and fixed gaze from a safe distance. No fear, but interest, approach, exploration up to touching and licking the glass box that contained the live snake clearly visible was found in 18 animals. Of these, 10 were infants under 3 years who all "kissed" the glass where the snake kept moving; 5 were immature animals of both sexes, 3 were old adult females. In the fear-exhibiting group, on the other hand, there was no infant, 12 were immature and 22 were adult animals of both sexes. There is thus some indication that fear of the snake grows with age. The fear is quite specific and is not elicited by lizards, toads, or large insects shown under the same conditions. After the initial tests of reaction, a series of training tests on the 32 animals which showed fear reactions was conducted as follows: A snake in a glass box with glass cover on which was placed a piece of banana was presented before the cage and left until the banana was taken. The box was then presented without food, and any fear reactions were recorded. Training and test presentations were alternated for 14 trials. Table 2 shows the number of animals that did not show fear reactions in the test presentations but responded by approaching and even touching the box.

TABLE 2

NUMBER OF APES DISPLAYING FEAR OF A SNAKE AFTER TRIALS ASSOCIATING SNAKE WITH FOOD

Before cond'g	After 2,	4,	6,	8,	10,	12,	14 trials
32	25	19	14	9	5	2	2

Twelve trials were sufficient to abolish the fear reaction in all but two of the 32 animals.

In a complementary experiment a banana was placed in the same glass box. The box was arranged with a wire cover which could be charged so that the animal received a shock if he touched a piece of food placed on the cover. Table 3 shows that the positive reaction to the food in this situation was extinguished in 8 trials in all the subjects.

There is no significant difference in

TABLE 3

NUMBER OF APES (OF 8) AVOIDING THE PRESENTED FOOD AFTER TRIALS OF PUNISHMENT FOR TOUCHING FOOD

Before cond'g	After 2,	4,	6,	8,	10 trials
0	3	5	7	8	8

the amount of training required to extinguish the fear reaction to the snake and the positive reaction to the banana. Since the banana is not native to the chimpanzee's habitat, responses to it are certainly not innately determined.

In these two experiments there is no learning differential between the responses to what has sometimes been assumed to be an innate perceptual schema and to an object to which the response has certainly been learned. There is no evidence in the ape of innate specific responses to specific perceptual configurations. Although there are genuinely preferred ways of reaction, native motor patterns can adhere to almost anything that fits a certain dimensional range. In Lorenz' terms it is the taxis, not the automatism that is variable. The variation in the motor pattern is restricted to its fuller or poorer release by external cues.

Are Unlearned Motor Patterns Conducive to Instrumentational Behavior?

The results reported in the previous section cast considerable doubt upon the value of visual organization as the sole guide to complex learning. It is an interlocked sensorimotor integration, rather, that seems to be responsible for adjustment. The sudden changes in perceptual organization, inferred from the insight type of problem solving, are not based on specific processes coordinated readily before any functional practice. The efficient relation between perceptual patterns and displayed motor patterns must be acquired in the course of use. As a matter of fact, we shall see that rather comprehensive motor complexes appear without any specific training, but their application to external situations is due to habituation or experience.

The pioneer studies on using a stick to obtain food beyond reach in the orangutan and the gorilla by Yerkes (43), in the chimpanzee by Köhler (19), gave the impression that a relational perception of food and tool is sufficient for the overt response that leads to a more intimate contact between these separately presented entities. Occasional observations of Jackson (17) and systematic experiments by Birch (3, 4), however, have shown that some experience with the handling of a stick is necessary for "insight" or "ideation" to be expressed. Both found rapid generalization, once an initial learning was obtained. The generalization was tested by Birch in single trials with 10 different stick problems, for each of which a full hour of trial time was allowed. Only 3 of the 10 problems were solved by all 6 animals used, and those who solved all the problems took an average of several minutes and only 12 of the total of 60 trials were positive within one minute. No immediate success was thus generally obtained.

3. I have analyzed the stick problem in 25 chimpanzees of 1 to 15 years of age. Contrary to Birch's findings I have found no significant improvement in the use of the stick by providing young animals with an opportunity to play with sticks for days or even weeks. On the other hand, a maturational gradient was found. Older animals solve varieties of the stick problem much more rapidly than younger ones. Even they require some *specific* training with the stick in order to use that tool efficiently. A gradual introduction of more difficult problems after the easier ones furthers the achievement.

I have built up a procedure, beginning in the youngest animals with a *string* baited first at the closest end within reach (a), then on the farthest end out of reach (b); after this the *rake* was introduced, with the food at first between the rake and the animal (c), later to the side of the rake. The

straight stick was first used in an *oblique* position, the food in front of it, so that its drawing in will bring the food closer (d); then it is *vertical* to the plane of grill behind which the animal is working and the food is within the sphere of sweeping an area of 90° toward himself (e); later the food is presented farther aside and even *behind* the stick so that the latter has to be adjusted to the location of the food before pulling in (f). After this adjustment has been made the stick is given *in the cage* and the food without, beyond reach of the naked arm (g). If this problem is mastered, the stick is put in a place where it cannot be seen simultaneously with the food and cannot be hit upon by accident (h). These two-platform experiments are continued after having introduced a *second,* shorter, *stick* that serves to procure the longer stick that is needed to get the food (i). The most complex problem studied utilizes *three platforms,* one for each of the two sticks and one for the food, all including an area of 270° around the chimpanzee (j). The ablest animals were given two sticks, one with a hollow, the other with a peg-like end so they can be *joined by insertion* to form an elongated single stick (k).

The five youngest chimpanzees, of the age of 1 to 2 years, learn this series only up to the point where they need not adjust the stick to a location of the food behind or beside it, that is through (e) of the above list. Even these simplest problems require a repetition of trials up to a few hundred, to be readily solved. It is very much like a result of conditioning, for the animals just learn to do something to the stick before obtaining food. The infants will try just to touch it, or pull in straight, or push it aside and display, after this performance, every sign of expectation of food. Since the effective sweep alone is rewarded (plus some "good" but by chance inefficient trials, in order to avoid discouragement), the best way of doing it will be gradually built up.

Six youngsters of 3 to 4 years of age learn essentially in the same manner but considerably faster. Roughly a hundred trials suffice to bring them to master even more adaptive problems, up to the stick in the cage that has to be put out to the food in a proper position to get it in, through task (g).

Eight chimpanzees of the age between 5 and 8 (probably corresponding to Köhler's animals) take roughly a score of trials, with the straight stick alone, to reach the level of the younger group and then proceed in another score of trials with a spread of generalization to solve the more complex tasks. These animals have no difficulty with multiple platforms. It is not so much the optical separation of the sticks and food that makes such tasks difficult, but the fact that a longer series of single responses has to be integrated into one complex pattern. All animals of the group solved two-stick problems (i; j) even on 2 and 3 platforms, the older ones certainly in fewer trials than the younger ones.

The group of 9 to 15 year old animals, pubescents and young adults, contained two of those used by Birch. All of them had to learn gradually, by specific experience too, stepping from the simpler to the more difficult problems, as did Köhler's chimpanzees. The experience which two of these animals had many years before did not show up in their performance. They were not advanced as compared to those closest in age to them. Even the adults needed almost the same amount of specific experience as the pubescent animals, but the former generalized faster. I started the series with this group by presenting to the animal food behind the accessible stick. All of them reached first for the food across the

stick and then, after some hesitation, took the stick and pulled it in with no reference to the food, licked, smelled, and chewed it. Much later only did they put the stick out again and push it toward the food. Then they suddenly swept or angled for the food, at third or fourth trial, and rarely made any more mistakes. Silly behavior sometimes occurred, especially as the multiple platform problems were introduced. Like the younger ones, these big animals tried to reduce their efforts and just push or pull a bit on the stick and then look alternately to the food and to the experimenter.

This series of tests shows that there is an important maturational element in the adaptive use of the stick as a rake by chimpanzees. Experience in the handling of sticks is necessary for the solution of the rake problems, but with equivalent amounts of experience in the use of the stick as a rake, the older animals advance to a more complicated level of performance than the younger. (The conditions of caging at Orange Park are such that even the adult animals tested have had practically no opportunity for practice in rake using prior to these experiments.)

The jointed stick problem gives especially clear evidence of the interrelations of maturation, experience and insight. Köhler (19) gave a classical description of a male chimpanzee, Sultan, aged about 6 years, who solved the drawing-in problem by putting together two bamboo sticks that matched on either end. Sultan's performance was first produced in play but was utilized "immediately" and was repeated the same and the next day several times, even with three sticks. Moreover, he pulled out a stopper that prevented joining the sticks before he attempted an insertion and bit off the two-broad end of a board to make it match the hole.

None of these activities need be regarded as having any reference to the problem situation. The manipulation of sticks by older animals that have had no prior opportunity of handling jointed sticks when there is no food to be reached shows all of these varieties of activity. Licking, chewing, stroking, and splitting the stick, banging, poking, hammering with it, and thrusting the end into any available openings are responses that occur frequently and constitute the basis of complex motor patterns of utilizing sticks as tools.

4. To test stick-joining without the motivation of the food and rake problem, two short sticks, of which the end of one could be fitted into the other, were given to each of 48 chimpanzees. Thirty-one of these fitted the sticks together in play within one hour (tests made in four 15 minute periods). Of 20 adults in the group, all but one (a pregnant female) joined the sticks repeatedly and the majority did this within the first 5 minutes. Of 16 animals between 5 and 10 years, 11 fitted the sticks together within 5 minutes. Of 12 chimpanzees less than 5 years old, only one joined the sticks within an hour. She was a home-reared animal that had been taught similar tasks.

The infants played almost exclusively with one stick at a time. Those of the bigger animals who did not join them showed, nevertheless, elements of this pattern by inserting a finger into the hole, putting the point of the peg into some crack in the floor or fence. Those who connected the sticks usually chewed the end of the peg before inserting it into the hole and after this they took the peg out and licked it. The hole was often filled with food, water, or dirt, and these substances were then sucked out of it. Some active chimpanzees repeatedly poked through the fence, shaking the sticks in the face of the experimenter or other animals. As

the experimenter approached, the chimpanzee withdrew suddenly as if chased, or prepared to attack with the other hand or both hands, if the stick were offered in the mouth. Sometimes they actually gave the stick and then presented back, shoulder, or rump to be scratched. All of these activities occurred in series of many repetitions before variation. Vocalization, especially that characteristic of grooming situations, frequently accompanied this social play and the exploration of cracks and holes by poking.

There are some correlations of the play activity with age and need factors. Older chimpanzees manipulated more persistently than the younger ones (see Table 4) and had a larger variety of manipulation forms. While younger animals handle the stick as a passive thing with no relation to other objects —just chew and scratch it, carry it or rest on it (with shoulder, neck, or head) —older ones use sticks to mediate activity, poke at or batter another object, eventually combining both sticks. The same patterns of handling objects constitute components of "instinctive" activities like investigation and grooming, building of twig-nests, and sexual games. There seems to be a difference in the frequency of types of manipulation according to the prevalent needs. Hungry animals lick and chew the sticks, and socially isolated ones perform mostly exploratory and grooming-like activities as described above. Raising the level of any particular activity by developing needs or habits may facilitate its utilization in the composition of problem-solving stereotypes.

Fitting the sticks together and using them as a rake are independent activities, not immediately associated. Six adult animals, which had united the sticks within 5 minutes in play and had joined and pulled them apart many times, failed to join them within three 15-minute test periods, when there was food to be reached. The frustration in the problem situation seems to inhibit the natural play activities. The behavior of one of these, Sinbad, a 12-year-old male, is typical of the reactions of the inexperienced chimpanzee when given the disjointed sticks with food on a platform beyond reach with the single sticks. Only on the fourth 15-minute trial did he join the sticks. He first tried to reach the food with a short piece but soon gave up attempts to get the food and resorted to play. He took both sticks into the cage, inserted the peg in the hole and had lots of fun holding the double stick upright, putting it to his shoulder and in his lap, rolling the constructed "tool" on the floor and even poking with it across the bars of the grill toward, but not for the food (several minutes of play). At the end of this session he accepted another, long enough, single stick and swept his food in cleverly. Play and work are distinct spheres of activity, not yet integrated at that stage. Next day he again began to play after a few minutes spent in the problem situation, but played only for one minute with the double stick, then he poked it through the grill and dropped it. He soon picked it up, placed its end behind the food, and swept in quietly (46 minutes total of 4 test periods). At next trial he made this whole sequence rapidly, so we could proceed to the multiple platform tests, which he solved with good adaptation, connecting the sticks in the cage between two reachings and sweeping in smoothly in about 20 seconds every time.

In none of the animals tested was there any evidence of the immediate perception of a relation between the united sticks and the distance of the food to be reached. Most typical was the joining of the sticks in the period of play, then pulling them apart and

TABLE 4

AMOUNT OF SPONTANEOUS MANIPULATION AND OF LEARNING NEEDED TO SOLVE A PROBLEM WITH A STICK

Animals	*Seconds* spent in spontaneous activity as indicated in a 15-minute period	Order of magnitude of Number of subjects		*Trials* taken to reach the indicated level of solution	
Spider monkey	30 stick passive	5	3	Straight pull only	500
Rhesus monkey	45 stick passive	5	2	Straight pull only	400
Infant chimps	120 stick passive	6	5	Straight pull only	200
2 to 4 years ch.	240 stick mediates	6	6	Stick adjusted	100
5 to 8 years ch.	540 stick combined	16	8	Multiple tasks	50
Adult chimps	600 stick combined	20	6	Multiple tasks	25

trying to reach the food with a short piece or with the bare arm. The discovery of its utility as a rake mostly followed its abandonment as an object of play.

The dramatic construction of a tower by piling boxes or cases "in order to" climb on top and jump for food suspended high above is most likely based on naturally preferred playforms also. I tested 12 chimpanzees of 6 to 10 years of age in two 15 minute periods with two of the smallest standardized boxes used by Yerkes and associates (44). All of them dragged the boxes along the floor, sat and stood on them, rolled them over, carried them carefully balanced to some preferred corner, and used them as pillows. Six of the animals actually stacked them and climbed on the tower, jumping upward from the top repeatedly, with arms lifted above the head and stretched toward the ceiling. For the human observer it was hard to believe that there was no food above them to be reached. Needless to say none of these animals had ever been tested in box-stacking problem situations. Nevertheless, the pattern of behavior, developing spontaneously in play without external motivation, duplicated exactly the pattern of using boxes to attain a suspended bait. Furthermore, as in the use of the jointed stick, the introduction of a bait interfered with and delayed the piling of boxes, just as it interfered with the playful joining of the sticks.

There is a definite relation between the amount and variety of non-motivated manipulation and the final achievements of the animals. This is illustrated by a comparison of monkeys and chimpanzees in the rake problem. I have tested 5 rhesus and 5 spider monkeys. They were given sticks and the time and varieties of play with the sticks were recorded. They were then trained to use the stick as a rake to secure food. All learned to sweep in the food when it lay between the stick and the cage. No one of them learned to get the food when it lay beyond the stick; they did not lift the stick over the food. Table 4 compares the amount and type of play activity with the stick and the subsequent training trials required for prompt and smooth use of the stick as a rake by monkeys and chimpanzees of different ages. The monkeys and infant chimpanzees played with the stick for only a few seconds before discarding it and the manipulation was limited to simple handling (passive) without use of the stick in relation to other objects. The 2 to 4 year old chimpanzees used the stick to poke at other objects (mediates) but failed to join two sticks. The older chimpanzees joined the two sticks in play. There is a perfect correlation between the average times of manipu-

lation without external motivation and the learning scores to achieve the highest level of problem solution attained by the group. As the functional abilities of the effectors develop, onto- and phylogenetically, the manipulation shows a higher degree of variety and longer series of activities.

It can be seen that the longer and more varied the spontaneous activity, the shorter the time for adjustment and the more complex the adjustment. Motor patterns at the disposal of the animal, whether learnt or unlearnt, enter complexes of response sequences that are, as they appear, more or less adaptive and become, by provoking repetition with incentives, solidified, smoothly running units of behavior. These adaptive complexes or generalized routines are conducive to problem solution. A compound operant is conditioned by the intrinsic consequences of the behavior proper. It is suggested that behavioral adjustment to environmental entities is a composite result of innate response patterns.

The innate constituents of complex responses are not perceptual organizations but motor patterns. The adaptive use of these patterns depends upon the span of activity that is composed and conditioned as a unit to some elicitor in a generalized fashion. Once avoidance, grasping, banging, poking, etc. are established, any familiar object in familiar surroundings that fits dimensionally to these activities will elicit them in a way that appears insightful. The positional relations of objects can be varied without breaking up the pattern, since it is the inhibition of repetitions and the orienting to subsequently produced cues that is learned.

DISCUSSION

There is no doubt that the motor components of any behavior compound are innately determined; probably larger series are established prior to learning in insects, fishes, and birds than in mammals. How large are the units, that is the question. What is a single response? One twitch, one flexion, one grasp, salivation of one drop, an orientational posture? The comprehensiveness of these innate entities is variable with age groups and animal forms. An analysis of the specific repertoire is needed before we can proceed to a learning theory which is more than a justification of one or the other very restricted method of behavior research.

These motor patterns are the basic units of overt behavior. There is *no* definite stimulus correlated to a more complex motor pattern. The releasers of a pattern may vary within a more or less broad range specific to the receptors of the species. Adaptation changes this range more widely in higher than in lower animals. The modification of behavior produced thereby seems to have two functionally consecutive phases: habituation and condensation. In the first a perceptual, in the second a motor factor is predominant. A duality of learning mechanisms, selective problem-solving as preceded by associative conditioning, was conceived by Maier and Schneirla (22) and, more elaborately, by Mowrer (25). Girden (9), however, asserts that the adaptive pattern is a function of the non-associative adjustment to repeated stimulation and thus there might be a continuity between the allegedly separate mechanisms. Habituation is necessary for condensation to take place. In conventional experiments we usually vary both factors at a time.

A unified learning theory can be attempted on the basis of regarding the original repertoire as consisting of operants. Their governing stimuli are internal. Hess and Brügger (15) discovered diencephalic loci, the direct

stimulation of which produces complex sniffing, fighting, and other sequences in cats. These patterns if emitted, however, are immediately applied to crudely fitting perceptual releasers. Such sham-adjustment can be a consequence of structural correspondence between dimensional properties of the effectors and the perceptual field. The centrogenic spitting of cats, e.g., was directed toward persons present in Hess' laboratory. An ape seizes a stick without primary or secondary reinforcement, just because it fits the size of his grasp.[2] In consequence of the response, minor possible coincidences of extero- and proprioception are presented and enable the animal to make more accurate adjustment. Excitement by shock or food creates an emotional climate in which motor complexes are associated to either preceding or following cues (Tolman, 41, recently showed the latter possibility). Maybe this is just another way of saying that motor patterns are activated more readily by stimuli to which the organism becomes sensitive. Anthropomorphically stated: By repetition a new aspect of the situation becomes functional. A piano player learns in a similar way to attend (without knowing it) to proprioceptive cues that enable him, in coincidence with synchronous external perceptions, to speed up a sequence produced originally by independent impulses.

Application of ever more compresive motor complexes to involve cue constellations is the basis of adaptability in higher organisms. In order to integrate such complex operants, their elements must be matured or otherwise acquired; the motor patterns must be at the disposal of the animal, the orientation accurate enough and the span of synthesis, persistence, or attention sufficiently comprehensive to permit larger entities of sequences to be built up.

The classical description of smooth and continuous problem-solving in the insight experiment is certainly valid. Its genesis, however, is obscure. My chimpanzees showed the same type of behavior, but they needed a series of specific experiences until it was established. The difference in their learning from some of Köhler's chimpanzees is most likely to be explained in terms of life history. The surroundings of the colony animals in Florida are much more monotonous than were those of the freshly captured Teneriffe animals, and the former surely do not have all the opportunities necessary for unimpaired functional development. In a natural habitat or free colony outside of experimental conditions the consequences of behavior follow the laws of reality. Thus the animal has no other choice but to adjust. The mechanism of adjustments, however, can only be seen in maladjustments, when the experimenter forces strictly controlled arbitrary consequences upon the animal.

It is the failure rather than the adjustment which is conspicuous. The mistakes my animals made were seldom of the "good error" type that indicates some intention on the part of the animal but, on the contrary, they mostly showed stereotyped routines of the sort produced by Guthrie and Horton (10) in cats or by Skinner (38) in pigeons. The stick is a token, the mere touch of which induces inaccessible food to become accessible. This "hypothesis" or "superstition" is soon established as a conditioned operant. It becomes reinforced chiefly in consequence of proper action with the stick and thus the proper components of the initial

[2] This statement is made deliberately in that absurd way to stress that there is no need for assuming either innate schemata or established connections to account for complex adjustment. There is a preference for larger sticks with growing age (and size of hand), preserved despite experience at variance with it.

partial responses become emphasized, their sequence is condensed. The chimpanzee will not poke repeatedly at the stick or push it back and forth twenty times, as in the beginning of his trials, but goes over to the next variety of available response and displays all these immediate responses in rapid succession, integrating temporally a piecemeal pattern into a more unified compound. The sequence becomes elicitable as a composite complex by the original releaser of the first member.[3]

There is no agreement as to the proper units of behavior to be utilized in analysis. While it is not questionable that the psychologist's main concern is the ultimate effect produced in the relation of the organism to its environment, this can only be analyzed in terms of actual activity of the effector organs. Complex motor patterns and their temporal sequences have varied correlates and mediators in the neural tissues. A single operant includes skeletal, visceral, glandular, chromatophoral, etc., responses. The behavior student has to relate something defined in the environment to something defined that the organism does thereupon and affects the environment in consequence. What happens in between, within the boundaries of the organism proper, is the physiologist's concern (29). Such an analysis contributes to the effort to correlate the environmentalistic behavior data to neurophysiological entities. This approach renders observed external relations valid and discloses the basis of behavior mechanisms (for a lucid presentation of this view see Pratt, 27). But the pure behavior description must advance first in order to be comparable to the well developed terminology and measurements in physiology. We must have a scientific inventory of the original behavior patterns, the modifications of which constitute the topic of learning studies. The hierarchy of available patterns, modified by physiological periodicity as well as by external activation, determines the probability that any pattern will be evinced in a particular situation.

Though paradoxically alien to psychophysiological aims, the Skinner program (37), analysis of the effects of restricted environmental influences, must be fulfilled before physiological interpretation. It is, of course, to be questioned whether Skinner's selection of units is a sound one. He has recently shifted from the study of a single routine to allow more freedom to his animals in action. As a result he noticed patterns of behavior that can be definitely stereotyped though they have no instrumental relation to the environment and resemble, rather, magic activities (38). These, like the instrumental activities, are compounds of innate responses, once emitted by chance at a time when reinforcement followed and thus established as "conducive" to reward.

Laws of effect, practice, and insight as concurrent alleged bases of adaptive behavior cannot be dealt with basically, unless the behavior facts prior to systematic training are analyzed extensively. The effect of not strictly associative factors like negative adaptation (11), or of other historical factors, like maturation on modifying behavior can be analyzed conclusively only under strictly constant life conditions.

Long range projects of research alone can determine what relations the animal's general experiences with its own

[3] Analyzing Thorndike's learning curves of monkeys (*Animal intelligence*, New York: Macmillan, 1911, pp. 185–187), it becomes clear that multiple manipulations, like his triple tasks, composed of three operations, were the most difficult ones to learn. The same effect is explicitly shown by N. Kohts, Les aptitudes motrices adaptives du singe inférieur. *J. de Psychol.*, 1930, 27, 412–447.

effectors and with the environment to be handled have in the cumulative process of development. Experiments on animals kept for years in isolation from distinct objects, deprived of society and other natural reaction-evoking agents, kept in artificial bandages to prevent the use of their extremities without anatomical damage, etc., have to be planned so as to enable analysis of the factors in question. It could then be seen how operants of the type of reaching, touching, grasping, sweeping, climbing, etc., develop with no opportunity for them to become associated with any definite perceptual patterns. Systematic reinforcement of selected motor sequences in the course of early life could show the limits of building up and generalizing stereotyped series. The framework of adaptability cannot be seen without prolonged experimentation, for the animal usually inherits not only his organism but also the environment in which he lives (23). Radical variation of persistent environmental factors is necessary to find out true constants in the native reaction potential.

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THE NEUROPHYSIOLOGY OF LEARNING AND DELAYED REWARD LEARNING

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In some earlier papers the writer endeavored to explain in neurophysiological terms the phenomena of stimulus patterning (22), reinforcement (23), and primary stimulus generalization (25). In the second of these articles the intention was expressed of dealing more fully later with the special case of delayed reward learning. To do so has now become imperative in view of Seward's recent attempt (19) to uphold an interpretation of delayed reward learning in terms of fractional goal reactions and secondary reinforcement. This interpretation conflicts with the present writer's neurophysiological theory of reinforcement (23), to which specifically Seward has raised certain objections. In the present paper this neurophysiological theory will be expounded in greater detail, so that its application to delayed reward learning may be seen as clearly as possible. But it is an essential preliminary to show, first, that the objections Seward has raised do not constitute serious difficulties for the theory, and, secondly, that there are considerations of fact that seem to make untenable theories of delayed reward learning that depend on the concept of secondary reinforcement.

Answer to Seward's Criticisms

The first thing to be made clear is that Seward is not criticizing neurophysiological theories in general, but a *particular* neurophysiological theory. He expresses his own theory of how learning occurs in terms of Hullian constructs—constructs that belong to the class that MacCorquodale and Meehl (11) have categorized as "a summary of empirical relationships"—but he would probably agree that if his version of the learning process is correct, it must also have a neurophysiological basis. His criticisms of the writer's neurophysiological theory take the form that certain of its implications are irreconcilable with certain relevant facts.

Seward's first criticism is that the theory "is based on the assumption that reinforcement is an increasing function of stimulus intensity." In point of fact no such assumption is made, and it is repeatedly emphasized (23) that amount of drive *reduction* is the factor that is correlated with amount of learning. Regarding those instances of learning in which the reduction of primary needs plays a part, it is not a necessary assumption that the amount of drive reduction consequent on the reduction of the need (by a given quantity of food, for example) will be greater when the initial level of need is higher. Whether or not experimental evidence showed this to be the case would make no difference to the theory. Drive reduction means reduction of central neural excitation, and it may or may not be that, for a given amount of a need reducing agent, this excitation will be reduced by different amounts at different levels of need.

Initial stimulus intensity matters more when learning occurs in the absence of the reduction of a primary need. In that case, as pointed out previously (23), some measure of drive reduction can be presumed to have occurred as a consequent of stimulus-cessation. The greater the initial in-

tensity of the relevant stimuli the greater will be the subsequent drive reduction. The question is, *what stimuli are relevant?* There appear to be three possibilities. We may consider them with reference to a situation in which a tone is to be made a cue to the responses already made to a flash of light. To this end the stimuli are repeatedly presented together. The drive reduction here subserving reinforcement may be (1) a joint function of the drive reduction associated with the cessation of the tone and of the light, *or* (2) a function of the light-cessation drive reduction only, *or* (3) a function of the tone-cessation drive reduction only. (Were either of the latter two possibilities to apply, the *other* stimulus-cessation would obviously be the relevant one insofar as the flash of light was becoming a cue to the responses already made to the tone.) The investigations cited by Seward (2, 5, 6) to show a lack of relationship between conditioned stimulus intensity and strength of conditioning have possible application only to the last of these possibilities—that is, where the cessation of the stimulus being conditioned (tone) is alone the source of the relevant drive reduction. In any case, the investigations cited do not really provide data concerning learning in the absence of the reduction of primary needs; for the "unconditioned" stimuli to the responses reinforced (eye closure and GSR) presumably produced some measure of primary need. It is worth noting that in the case of eye closure Passey (15) has demonstrated that rate of conditioning is an increasing function of the intensity of the unconditioned stimulus.

Seward's second criticism is an attack upon the assumption that reinforcement is a decreasing function of the time interval between synaptic activity and drive reduction. He says: "It follows that buzzer-shock conditioning should be less effective if the buzzer ceased at the onset of the shock rather than at its termination." This does not follow at all, because if the buzzer continues after the onset of the shock the far more powerful responses to the shock would inhibit the responses to the buzzer. Thus, even though the buzzer continues after the onset of the shock its activating effect on synapses is diminished or perhaps eliminated. This view gains support from certain observations of Gellhorn (4) and Barker and Gellhorn (1) recently brought to the attention of psychologists by Jones (10). These workers showed that nociceptive impulses excite cortical suppressor areas, and this suppresses or diminishes the cortical effects of other afferent impulses. It would accordingly be expected, contrary to Seward's view and Mowrer's (14), that the longer the shock lasts the less effective will conditioning be, even though the buzzer is concurrent with it, because *effective* buzzer action on synapses will be farther removed in time from the drive reduction due to cessation of the shock.

CRITICISM OF SECONDARY REINFORCEMENT THEORY OF DELAYED REWARD LEARNING

Seward's version of this theory is the most sophisticated and elaborate to date. Taking as an example the case of a hungry rat in a simple T-maze with food in the right end-box, Seward assumes, with Spence (20), that "at the moment of finding and eating food there are proprioceptive consequences of turning right still reverberating in the rat's brain. They are thus conditioned to the consummatory response (R_G). When next the rat starts to make a right turn at the choice point, proprioceptive impulses are again aroused which, by stimulus generaliza-

tion, evoke some fraction (rs_G) of the conditioned response to food" (19, 365). The rs_G thereupon reinforces the response in progress in proportion to its own intensity.

It should be clearly noted that according to the above theory it is only *after the animal has turned right during his second run* that reinforcement of right turning occurs for the first time, for not until then are the proprioceptive cues evoked which in turn evoke rs_G. Thus, at the second run there should not yet be any increased tendency to turn right. The literature does not seem to record any direct observations on this point, but Reichlin's observations on what happens during extinction give us significant indications that the right turn is in fact reinforced *during the run in which it is followed by reward*. Seward's theory requires that right-turning will be reinforced during a run if the *preceding* run has been rewarded by food, and *even though no food follows*. Consequently, if, after a number of rewarded runs, one is not rewarded, the run that succeeds the unrewarded run should not reveal any weakening of the right turning tendency.

This expectation is strongly contradicted by observations made by Reichlin (18) in the course of an investigation on vicarious trial and error behavior. In her Experiment No. 5, hungry rats were trained to jump once daily from a platform to the right or the left entrance of a rectangular maze, obtaining 2 grams of food at a food box 4'6" from the entrance. After 12 days of this preliminary training the food reward was suddenly cut down to 0.4 gram. Continuing this small reward day after day produced a tendency towards extinction of the jumping, but already on the day after 0.4 gram was given for the first time the mean reaction latency for 24 unselected animals had risen from 6.5 seconds to 21.4 seconds. Clearly this could not have occurred if the first jump to be followed by 0.4 gram had been reinforced by the rs_G previously conditioned.

Further experimentation is, of course, needed; but this experiment seems to the present writer to cast the gravest doubt on the whole theory of secondary reinforcement as mediated by fractional goal reactions.

A NEUROPHYSIOLOGICAL HYPOTHESIS OF LEARNING[1]

In the article on reinforcement referred to above (23) reasons were given for concluding that learning depends upon the formation or strengthening of synaptic connections at points of anatomical apposition of neurones, a synapse being a *functional* point of contact between neurones. As far as learning theory is concerned, the most important question to which neurophysiology could give an answer is, what happens at the synaptic points when learning takes place? We are still far from a complete answer, but it is entirely feasible to attempt to work out a hypothesis that

[1] The hypothesis that follows is not contradicted by Lashley's contention (*Symposia of the Society for Experimental Biology*, London: Cambridge Univ. Press, 1950, No. 4, pp. 454–482) that no individual neural elements in the brain, apart from purely sensory ones, are essential for given learning or retention. As mentioned previously (23), one instance of learning has been shown experimentally to be correlated with the presence of neural interconnections that do not exist either before the learning or after its extinction. If this kind of correlation applies to all learning, Lashley's findings merely imply that multiple neural connections *in parallel* in various parts of the brain subserve the same learning. Whatever the details of neural organization may be and however great its complexity, learning seems to have the basis that the neurones responding to a given sensory constellation somehow become functionally connected to the neurones subserving a given response.

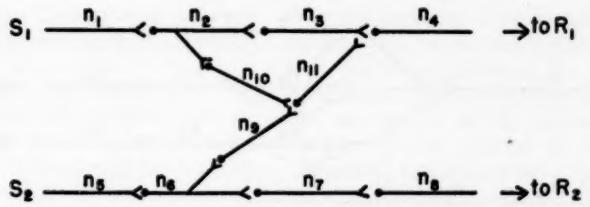

FIG. 1. Diagram illustrating the mechanism of reinforcement in situations of the Pavlovian type.

will do as a provisional account of what goes on, until future research places at our disposal the findings of more intimate studies of the relevant synaptic processes. Meanwhile the hypothesis gives rise to expectations which may be subjected to experimental tests, the results of which may either support the hypothesis or require modification of it.

As an aid to clarity, and not because of any supposed difference in essence, Pavlovian conditioned response learning and instrumental types of learning will be dealt with separately.

1. *The neural mechanism of conditioned response types of learning.* In the case of Pavlovian conditioned response learning the hypothesis is outlined with reference to Fig. 1. In this figure, S_1 represents a repeatable stimulus energy (such as an electric shock applied to a foot) that evokes responses serially in neurons n_1, n_2, n_3, and n_4, culminating in a movement R_1 (e.g., lifting of the foot). A stimulus energy S_2 (e.g., the sound of a bell) leads in a similar fashion through n_5, n_6, n_7, and n_8, to a movement R_2 (e.g., turning of the head in the direction of the bell). The neurones n_{10} and n_{11} are components of an internuncial chain along the pathway S_1–R_1; and n_9 is a neurone that receives impulses from n_6 and terminates in anatomical apposition with n_{11} but without forming a sufficiently effective functional connection for impulses from n_9 *alone* to be able to initiate responses in n_{11}. Thus, the neurone n_9 forms merely an *anatomical link* between the series S_2–R_2 and the series S_1–R_1.[2]

Now, the occurrence of R_1 (foot-lifting) leads to a removal of the stimulus conditions S_1 and their consequent neural excitations. Let it be supposed that on several occasions there is a temporal contiguity between the sequence S_1–R_1 and the sequence S_2–R_2, and that it is subsequently observed that a tendency is being built up for S_2, *when it occurs alone,* to be followed by R_1 as well as by the usual R_2. It is a clear inference that what underlies this increasing tendency is some change at the point of anatomical contact between n_9 and n_{11}, which has the effect that at least some impulses passing along n_9 are enabled to evoke a response in n_{11}, which in turn stimulates n_4 and hence the remainder of the series to R_1. The fact that the point of contact between n_9 and n_{11} has become capable of transmitting impulses from n_9 to n_{11} gives that point the status of a *synapse.*

In the setting up of the functional connection at the $n_9 \cdot n_{11}$ junction, both the S_1–R_1 sequence and the S_2–R_2 sequence seem to be necessary, and each must therefore be regarded as having some essential effect at the $n_9 \cdot n_{11}$ junction. Since amount of reinforcement is

[2] The great wealth of ramifications in the nervous system makes it reasonable to suppose that such anatomical links, having the potentiality of becoming functional, interconnect any two *S–R* series of neurones.

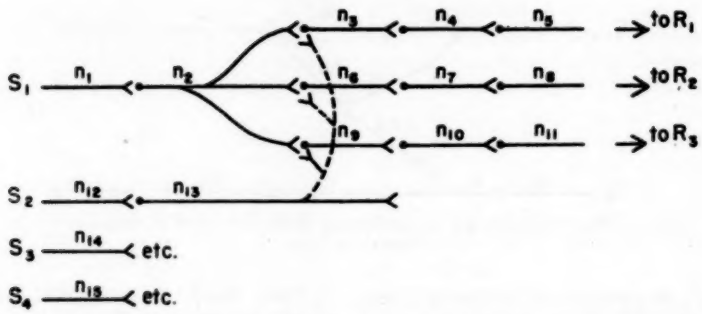

FIG. 2. Diagram illustrating the mechanism of reinforcement of an instrumental act.

related to amount of drive reduction, the greater the drive reduction that occurs in the S_1–R_1 sequence the greater may the change at the $n_9 \cdot n_{11}$ junction be expected to be. In the present case, the strength of drive in the S_1–R_1 sequence is determined by the strength of the electrical stimulus S_1. To the synaptic zone that includes the $n_9 \cdot n_{11}$ junction, impulses from the electrical stimulus arrive through n_{10}. The frequency of these impulses will vary directly with the strength of the electrical stimulus. If now, these impulses should cease to arrive, it is assumed that certain changes will occur in this synaptic area, and *the greater the initial frequency of impulses, the more marked will be these changes.* The magnitude of these changes is a determinant of the degree to which a functional connection is set up between n_9 and n_{11}.

But a functional connection will not be set up at all unless impulses from n_9, reaching or having reached the $n_9 \cdot n_{11}$ junction, bring into being certain preconditions for the formation of a connection. When we have discussed the case of instrumental learning it will be seen that these preconditions are not established by the mere *arrival* of impulses from n_9 at this junction, but the impulses must have been followed by a *response* in n_{11}, which would, to begin with, be produced by impulses from n_{10}.

2. *The neural mechanism of instrumental types of learning.* With the help of Fig. 2 the hypothetical mechanism of learning in the "instrumental" type of situation will now be described. Let S_1 represent a continuing stimulus energy, such as the electric charge in the grid of a cage shocking a rat. Let S_2, S_3, S_4 represent other stimulus energies to which the organism is responding. S_1 results in the passage of a continuous series of impulses along the neurone n_1 which transmits them to the neurone n_2. Now, n_2 has synapses with a number of neurones, n_3, n_6, and n_9 so that the chains n_3–n_4–n_5, n_6–n_7–n_8, and n_9–n_{10}–n_{11} are each stimulable, and stimulation of each is correlated with a different motor act—R_1, R_2, and R_3, respectively. Which of these chains responds to the impulses from n_2 at a given time will depend upon the conditions that exist at the synapses $n_2 \cdot n_3$, $n_2 \cdot n_6$, and $n_2 \cdot n_9$.[3] Certain of the factors that determine synaptic conditions were discussed in a previous article (22), and here it need only be added that in the present instance impulses

[3] The order of occurrence of the responses will depend partly on relative strength of initial connections. Hull (8) has termed this order the "habit family hierarchy."

consequent on the stimulus energies S_2, S_3, S_x will be among the determining factors.

Suppose that the chains n_3–n_4–n_5, n_6–n_7–n_8, and n_9–n_{10}–n_{11} have been activated in the order stated. Suppose that after the activation of the first two chains S_1 continues to act, but immediately after the activation of n_9–n_{10}–n_{11} the action of S_1 ceases (because R_3 removes the animal from the action of S_1). The next time S_1 stimulates the organism in this situation, there will be an increased *tendency* for n_9–n_{10}–n_{11} to be activated rather than n_3–n_4–n_5, or n_6–n_7–n_8.

In discussing the mechanism of Pavlovian conditioned response learning it was suggested that at a synapse that has been made "susceptible" by certain prior conditions, a cutting-off of a stream of impulses results in some change that has the effect that subsequently impulses can pass across this synapse more easily.[4] In the case of Pavlovian conditioned response learning, the prior conditions seemed to be determined by a discharge of impulses from the neurone whose linkage at the synapse concerned was about to be strengthened. In the present case, the mere discharge of impulses from n_2 could *not* have produced the necessary preconditions for reinforcement. If it had, reinforcement would have occurred in equal measure at synapses $n_2 \cdot n_3$, $n_2 \cdot n_6$, and $n_2 \cdot n_9$; whereas, in fact, reinforcement has been greatest at $n_2 \cdot n_9$. This synapse differs from the others only in that impulses were *transmitted* through it *more recently*.

Thus, the generalization may be made, that, at a synapse, the shorter the interval between the time of the *last transmission* of impulses and the time at which impulses *cease to arrive*, the stronger will be the functional connection established between the neurone from which impulses were arriving and the neurone to which impulses were transmitted. In relation to this generalization, two points must be noted. First, reinforcement can occur even if the reduction of drive does not amount to complete cessation. Second, in the above diagrammatic instance, any other neurone, for example, n_{13}, that makes contact at the point $n_2 \cdot n_9$, and that has recently delivered impulses to this point, will, when the drive reduction occurs, also be left with a strengthened functional connection to the post-synaptic neurone (n_9).

Hence, the following broader generalization may be formulated: Whenever the impulses, arriving at a *synapse that has transmitted impulses*, become significantly diminished in number, functional connections to the post-synaptic neurone are reinforced to all neurones which have recently delivered impulses to this synaptic point.

DELAYED REWARD LEARNING

We are now in a position to see clearly how drive reduction subserves the reinforcement of stimulus-response sequences that have occurred a considerable time before. When the rat in the T-maze has just turned right at the choice point, numerous neural impulses from many stimulus sources (e.g., hunger, sight, proprioception) have been arriving at points of linkage with neurones subserving the right-turning reaction. The necessary preconditions for synapse formation are thus already present, and the process will come to fruition if a strong reduction of central neural excitation occurs within a limited time—before the preconditions have faded too much. (Impulses from all the above stimuli are also arriving at inactive motor points,

[4] It is not implied that this can happen at all synapses. Some synaptic connections are probably maximally established by maturation, and unmodifiable.

but there, of course, according to our hypothesis, the pre-conditions for reinforcement do not exist.) While other stimulus conditions are changing from moment to moment as the animal moves, the hunger stimulus is continuously the source of high frequencies of impulses at all possible neural linkage points, and when these impulses are diminished by the animal eating, reinforcement will occur at all linkage points at which the necessary pre-conditions persist sufficiently. At such points functional connections (synapses) will be developed to a degree that is in inverse relation to the length of the interval between the production of the pre-conditions and the drive reduction. The shorter the interval, therefore, between the rat's right turn in the T-maze and the eating, the more will be increased its right-turning tendency in response to whatever special stimuli occur at the choice point; and if left-turning preceded right-turning, left-turning will not be as strongly reinforced.

A major implication of the present paper, as of its predecessor (23), is its denial of the occurrence of what Hull (9) calls secondary reinforcement as a distinct variety of the learning process mediated by fractional goal responses. According to the present neurophysiological hypothesis, the temporal gradient of reinforcement can be conceived of as wholly primary. This fits in particularly well with the remarkably clear-cut observations of Perkins (17), who was himself only deflected from such a conclusion by consideration of the "much shorter gradient obtained by Perin." Too much importance has been accredited to Perin's results (16), for they are complicated by the fact that his animals, while waiting for their food, performed numerous other responses in practically the same stimulus situation as accompanied the bar-moving that Perin was studying. Such responses would be more frequent and more likely to be overwhelmingly closer to the reward when the delay was longer, and consequently would receive stronger reinforcement than the bar-moving would as a response to this stimulus situation. It is thus not surprising that Perin found that many of the animals of the 30-second group ceased to operate the bar at all after a varying number of trials.

Even Grice's apparent confirmation (7) of the role of secondary reinforcement in delayed reward learning is not really a confirmation, for his results can be quite simply explained otherwise. Employing a visual discrimination at the choice point, Grice found that if the goal box has the same color as the positive stimulus, learning at the choice point is greatly facilitated, and concluded that this is because the color gains great secondary reinforcing power as a result of its contiguity with eating. The same facts can be explained as follows. The rat sees a white goal box, enters it, and soon after eats. By repetition, a habit of approach to white is built up, and the increasing tendency to go to the white alley at the choice point is simply a matter of generalization of this habit.

Finally, it is necessary to suggest why delayed reward learning is faster when there are constant (and especially, differential) stimuli in temporal or spatial relation to the reward, for example, a distinctive goal box (3). It would appear that stimuli constantly related to hunger drive reduction (eating) become capable of increasing the magnitude of hunger drive by the addition to the primary drive of measures of learned or secondary drive, just as stimuli that have been contiguous with anxiety drive reduction become themselves capable of evoking anxiety drive (12, 13, 21, 24) —and be it noted that they do *not* be-

come capable of evoking anxiety drive reduction (goal response). It is suggested that where secondary drive enters thus into the situation, a given amount of food brings about a greater amount of drive reduction and therefore more learning than where primary drive reduction is alone involved. This proposition awaits experimental test.

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DELAYED REWARD LEARNING

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My purpose in this brief note is not to reply to Dr. Wolpe's foregoing paper (3) as a whole, but simply to correct an apparent misunderstanding of my view. Since I obviously did not make my position clear in the article referred to (1), others may well share the same misconception. Briefly, it concerns the question of whether reinforcers strengthen connections or facilitate responses.

Wolpe criticizes secondary reinforcement theories of delayed learning in general, and mine in particular, by deducing certain consequences and testing them against available facts. Taking the case of a rat in a single-unit T maze with a homogeneous environment, he contends that such theories imply two necessary outcomes: (1) no reinforcement is demonstrable until *after* the second trial on which the reinforced response occurs; (2) the effect of non-reinforcement is also not demonstrable until after the second unrewarded response. The reason for the lag is that under these circumstances it takes two trials for a single reinforcement to take effect: one trial to make proprioceptive cues secondary reinforcers by conditioning them to a goal response, a second trial to produce these cues so that they can strengthen the response itself. Only on the third trial will this strengthening show up; moreover, it will inevitably show up whether or not the second trial is rewarded. Against the latter deduction Wolpe offers Reichlin's data as an indication of a marked increase in latency on the *second* trial of extinction.

Wolpe's point is crucial for any theory in which *reinforcement* means the strengthening of an *S–R* connection. His criticism may indeed apply to Spence's (2) theory of delayed reward learning. Unfortunately, in pointing out the similarity between my position and that of Spence and Hull, I failed to make clear the difference. That omission I should like to remedy here.

What I meant to say was this: Although I agree with Spence and Hull on the importance of the antedating goal reaction (rs_G) as a reinforcing agent, I disagree with them as to how it operates. Reinforcement, to me, is essentially a principle of performance rather than learning. In this respect I share Tolman's position. Like his *significate*, my rs_G, once aroused, acts to direct the behavior in progress at the moment. In terms of this theory let us examine our hypothetical rat-at-a-choice-point more closely in order to see how Wolpe's objection can be met.

Trial 1. Rat turns right, proceeds to goal box and finds food. Stimulus pattern, including hunger drive stimulus (S_D) and proprioceptive impulses from right turn (rs_R), is conditioned to eating response (R_G).

Trial 2. As rat approaches choice point, rs_G is already aroused by direct conditioning to S_D and stimulus generalization to intra-maze cues. Rat *starts* to turn right at choice point (VTE), thereby arousing incipient rs_R, which combines with S_D to strengthen rs_G. This "tertiary motive" (1) in turn facilitates response in progress and rat completes right turn.

Thus we see that rs_G exerts its effect *on the very next trial* after the primary reinforcer is presented. As to extinction, although my theory does not yet

include a formal account, I expect that it will eventually handle Reichlin's results in a similar way. On the first extinction trial a "non-goal" or "frustration" response is presumably conditioned to proprioceptive cues just as R_G was earlier. On the second extinction trial a "surrogate non-goal response" is similarly aroused on the starting platform, partially counteracting the rat's previously acquired tendency to jump.

A word as to terminology is in order. The misunderstanding that led to the present communication can be traced in part to the ambiguity of the term *reinforcement*. Elsewhere (1, p. 363) I suggested an operational definition as follows: *When a stimulus change X, following a response R to a situation S, increases the probability of R to S, X is called a reinforcer and its presentation is called a reinforcement.* The term is frequently used, however, to refer to one inferred process by which response probability may be increased, namely, the strengthening of what Wolpe calls a functional neural connection or what Hull calls a habit. We already have a good word for such a process: the time-honored concept of *association*. It should be noted that the two definitions are not indissolubly linked. The same operation may produce the same end result, not by conditioning the response, but by selecting it from a number of possible alternatives. In this case the mediating process seems to be closely allied with the physiological concept of *facilitation*. If, at the present stage of our knowledge, we make reinforcement synonymous with either process, we beg a basic theoretical question.

In the hope that this note will actually help to clarify rather than confuse, I should like to make the following proposals: (1) that the term *reinforcement* be given the restricted meaning of a set of experimental operations; (2) that any inference as to the nature of the intervening process be given a distinct and appropriate label. For the two mechanisms under discussion I suggest the terms *association* and *facilitation*.[1]

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[1] Of interest in this connection is Wolpe's closing paragraph on the role of "secondary reinforcers" in delayed reward learning. His suggestion that they arouse secondary drive is surprisingly like my notion of "tertiary motivation" (1). But whereas my tertiary motive *facilitates* the response in progress, Wolpe's secondary drive *associates* by providing "a greater amount of drive reduction and therefore more learning."

MAIER'S BEHAVIOR THEORY [1]

BY GEORGE A. AUSTIN

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Now that Dr. Norman R. F. Maier of the University of Michigan is no longer personally active in the psychological laboratory, it seems fitting to criticize his work as a whole—in particular, his work in behavior theory.

Although Hilgard (3) and Johnson (5) have given some prominent mention in their reviews to Maier's activities, a general summary and evaluation has not appeared. I have endeavored in this paper to discuss his distinctive views and contributions on motivation, perception and association, and to examine his basic approach to behavior theory.

Maier's Basic Approach

The main formal characteristics of his approach, according to Maier (21), are emphasis on qualitative distinctions and emphasis on physiological processes. It would seem that every theorist must make qualitative distinctions. For instance, Skinner differentiates reflex strength from reflex reserve, since each one obeys different quantitative laws. In his writings Maier gives the impression at times that he thinks quantitative work in psychological experimentation is unimportant. He does agree, however, that once distinctions have received extensive and uncontroverted experimental support, then the variables distinguished must undergo more intensive quantitative investigation.

Dr. D. G. Marquis points out (24) that Maier's program of making distinctions has the effect of splitting behavior theory into parts—frustration theory, learning theory, reasoning theory, etc., —each having its own concepts, laws and range of situations to which it applies. Tolman has recently urged that there is more than one kind of learning (32).

Maier's argument for processes as the proper subject matter parallels Lewin's argument for studying genotypes rather than phenotypes of behavior. It is not the situations that are crucial—problem box, temporal maze, delayed reaction, tool-using, *Umweg*—but rather the inner workings of the organism as it endeavors to cope with the situations. It is difficult if not impossible to infer a process from behavior in a very limited range of experimental set-ups. But one can take similarities and differences observed in a variety of situations, sharpen them in a series of experiments, and with some confidence infer the underlying process at work. This Maier has tried to do in his research on reasoning, equivalent stimuli and frustration.

Emphasis on processes as physiological is just returning to psychological theory. Both Tolman (31) and Hull (4) stressed the molar as distinct from the molecular aspect of their approach. Historically this was a step forward. As is pointed out by Koffka (8), however, the identification of "molecular" and "physiological" is false and misleading. The organismal concept, one of the basic principles of biology, insists that cells and cell-groups are interdependent in their behavior, and subordinate it to that of the whole organism.

[1] My primary debt is of course to Dr. Maier. Valuable criticisms were given by Dr. D. G. Marquis and Dr. E. L. Walker.

It is also contended that neurological data are as yet insufficient to serve as the mainstay for behavior theory. Workers in neurology and psychology are certainly aware of the gaps in their respective fields of knowledge. But enough data are available for men of the caliber of Hebb and Lashley, to mention only two besides Maier, to feel justified in attempting to correlate neurology and psychology. It seems unwise not to anticipate the eventual joining of the two fields and not to take heuristic as well as factual advantage of neurology.

Motivation

Maier distinguishes four main kinds of behavior under the general topic of motivation. As noted above, each of these must obey its own individual laws.

(1) Behavior determined by "neural connections" only. This is exemplified by the startle, knee-jerk and saliva-flow reflexes, whether innate or conditioned. Typical laws are extinction and secondary conditioning (20).

(2) Motivated or (following Tolman) purposeful behavior. Such behavior, which Maier calls motivated in the strict sense, is characterized by a need such as hunger or prestige, and a goal such as a food box or public recognition where the behavior in question has previously been experienced (except in the case of "reasoning") as a means to the goal. This statement merely attempts to express more precisely the conventional definition of motivated behavior.

A special feature of Maier's theory is his insistence on a need for variability (17). Similarly, Tolman (31) remarks on the "multiple trackness" of behavior. Although this need can be masked, Maier sees stereotypy in behavior the exception rather than the rule. The counterpart of this doctrine is emphasis on the central role of attention (22, Chap. XVIII).

Although Maier seems to have good evidence for asserting the existence of a need for variability, he has not indicated the neurophysiological process behind it. Some notion of the process may perhaps come from Hebb's (2) theory of nerve cell-assemblies and phase sequences. Hebb writes (p. 227): ". . . the phase sequence continually needs new content to maintain its organization and persistence," so that a certain amount of psychologically novel stimulation is required to maintain an established phase sequence. Without stimulus novelty, new phase sequences result, producing variable behavior.

How Maier can call such behavior "motivated" poses a problem, in view of his strong stand against "after-the-fact analysis" in the search for goals (20). Certainly that aspect of the environment operating as goal-object for variability has unique properties (21); it "satiates" almost instantly, and it serves on the first trial as something to be approached and on trials following soon after as something to be ignored or avoided. Behavior driven by the need for variability is therefore connected with goals in a way that frustrated or simple reflex behavior is not, but it is not connected with goals in the same way as is trial-and-error behavior. Whether this is reason enough to classify it under motivated behavior in the narrow sense, as Maier does, is hard to say; one hopes for more clarity on a point said to be so distinctive and important.

(3) Variability of need-driven restlessness. Maier sees the first actions of an organism in a training situation as variable, propelled only by the needs of the organism and hence, by definition, unmotivated. This random variability of movement is subsequently

grooved by learning to more systematic variability (e.g., Krech's "hypotheses") or to actual stereotypy. Little is known of the laws governing random and systematic variability, and presumably it is not always possible for an observer to distinguish between them in concrete instances. Much problem-solving behavior classified as trial-and-error is goal directed, and must therefore by definition be motivated.

Maier defines motivation in several ways. Trial-and-error restlessness shows the inadequacy of one of them: ". . . we shall use the term *motivation* to characterize the process by which the expression of behavior is determined or its future expression is influenced by consequences to which such behavior leads" (20, p. 93). It has already been stipulated that restlessness is unmotivated, but certainly its future expression is in many cases influenced by the consequences to which it leads.

Frustration

(4) Frustration is likely to result when the organism has a strong need or combination of needs which it is unable to satisfy. Maier has attempted to define the process underlying frustration only in a negative way: "We shall use the term *frustration* to characterize the process whereby the selection of behavior is determined by forces other than goals or mere neural connections" (20, p. 94). As so defined, frustration seems to include trial-and-error restlessness, and it leaves no room for future distinctions within the general concept of motivation.

Characterized positively, the frustration process appears to dominate when an organism's needs become so great that its behavior, formerly aimed at satisfying a particular need or needs by consummatory action with an appropriate goal object, is now aimed merely at relieving tension *as such*. There is a threshold beyond which motivation ceases to act and frustration assumes control; frustration varies in strength (20).

Why does one type of behavior rather than another occur in a given frustration situation? Maier says that the organism employs the most "available" response, and remarks that "The concept of availability is extremely broad, since many factors in heredity, growth and learning are included" (20, p. 165). Like "preference," then, a concept Maier frequently uses to explain a given motivated response, availability occupies a position in behavior theory similar to that of *"Prägnanz"* and "isomorphism," Tolman's "least action" and Hull's "reinforcement." That is to say, availability is defined to be the principle of response selection when the organism is frustrated. That such a principle shall not become so broad as to be meaningless, rather rigid specifications must be made of the particular empirical laws subsumed under it.

Fixation. Maier has found (20) that the frustration stereotype is most likely to result in the laboratory when an animal is placed in a problem situation (1) with a driving stimulus to force *some* response, and (2) punishment of a large proportion (say 50 per cent or over) of elicited responses, (3) in which the animal is unable to find the "right" response.

Stereotypy resulting from frustration is unusually rigid in the sense that it is not basically influenced by rewarding a competing response, and is not only not weakened but actually strengthened by direct punishment. Examples of fixation resulting from frustration may be neurotic compulsion, ritualism, obsession, phobia, habitual criminality. Maier believes he has detected hitherto unrecognized evidence of abnormal fixa-

tion in reports of Hamilton (1), Newcomb (25), Patrick (26), and Sampson and Schneirla (27).

Aggression. In line with his general thesis, Maier argues that aggressive behavior serves primarily to relieve frustration tension as such, the form and recipient of aggression being secondary. The proper remedy for aggression is not punishment, therefore, but first, the direction of aggressive behavior into relatively harmless channels to reduce tension, and second, the removal of the cause of frustration by changing the need or situation or by inducing problem-solving behavior to bring about motivated responses and perceptual reorganization. Non-directive counseling procedure is seen as following this program by concentrating at first in therapy on catharsis and later on insight.

Convulsion, seizure. On this topic, Maier concludes after a review of controversy and experiment: ". . . it seems to the writer that seizure and convulsive phenomena occur under conditions similar to those which produce frustration-instigated behaviors. However, the piling up of the tensions (cortical excitations) is either so rapid or the relief mechanisms that frustrated behaviors afford are so ineffective that excitations accumulate to the point that the organism cannot withstand them. When this occurs the excessive tensions break forth in a mass reaction" (20, p. 139), likened to Goldstein's "catastrophic reaction."

Many questions of fact and interpretation (e.g., regression, anxiety) remain. But the concept of frustration-instigated behavior as being without goal is highly distinctive, and suggests re-examination of many accepted data and principles.

Perception

Perception plays an essential part in Maier's thinking. He accepts wholeheartedly the Gestalt principles of organization, although he does say, departing somewhat from the Gestalt doctrine, that such organization may be influenced strongly by learned as well as by native factors.

Perception influences behavior because, first of all, it determines what can be associated. But Maier goes so far as to assert that perception is a *cause* of behavior, and tries to show that many types of behavior obey the laws of perception. For example, attitudes are said to follow perceptual laws. Evidence from industrial psychology and individual therapy appears to indicate that change in goal-directed attitudes does not follow ordinary laws of frequency and recency, but rather the (non-continuity) laws of perceptual reorganization.

According to Maier, perception is best studied by Klüver's method of equivalent stimuli. Besides attitudes, Maier goes on to subsume under the equivalent stimuli mechanism transfer of training, generalization, delayed reaction, Hull's concept formation, and behavior in Yerkes' multiple choice problem (22).

The "common element" theory of transfer as well as the Hull-Spence gradient theory of generalization are rejected because they ignore the factor of organization (21). The allegedly "complex" behaviors are discussed at length (22, Chap. XX). Maier first assumes that no form of behavior should be explained by a "higher" process when it can be explained by a "lower" one. He repeats, criticizes and rejects the arguments that delayed reaction, concept formation and multiple choice are due to any process of "symbolization" or "abstraction." He then proceeds to show that the simple principles of perceptual selection, organization, and re-arousal are sufficient explanations in each case.

It may well be that the laws of perceptual organization and the physiological processes underlying the choice of equivalent stimuli play the important role attributed them by Maier. But however convincing this view may be to him, more facts and theory are needed before it can become very useful to psychologists with other theoretical inclinations.

Association

(1) *Learning.* The basic law of learning is formulated as follows: When two phenomena are experienced closely together in time, the later experiencing of one of them tends to reinstate the experiencing of the other. Association is thus sensori-sensory by contiguity, and may be of varying strength, meaning that the experience of one thing is more or less likely to reinstate the experience of another associated thing.

Maier and Schneirla (23) interpret many experiments in support of the *S–S* position. Kendler and Underwood (7) submit alternative interpretations on Hullian principles. They make two differing behavioral predictions along the line of these experiments, but refuse to discuss questions of cortical physiology. However, among the experiments cited by Maier and Schneirla, those of Leuba (10), Light and Gantt (11), Loucks (12) and Shipley (30) present especially strong evidence against the working of an *S–R* physiological process.

Also, *S–S* is held to explain more plausibly than *S–R* why high-order conditioned responses have a longer latency and smaller amplitude than lower order conditioned responses. Maier does not say that a response as such cannot be associated. He holds explicitly that rewards, punishments and responses as well as simple presented stimuli are associable. In fact, he is one *S–S* theorist who has written within his theoretical framework on motor skills (19, 22). But the *mechanism* by which these experiences are associated, he maintains, is *S–S*.

(2) *Understanding.* Maier is in substantial agreement with Katona's later statement (6) that "Understanding organized wholes is the prototype of learning," that blind or rote memorizing ". . . is resorted to under circumstances which are not suitable to understanding." Rote learning lies at the lower end of the continuum of meaningful learning.

(3) *Reasoning.* A distinction is made by Maier between learning and reasoning on the formal and procedural ground that in reasoning the subject associates things which he did not experience simultaneously. Problems are available which test only learning (e.g., discrimination box or variations on it) as are problems which are almost pure tests of reasoning (13, 14, 15).

Empirical support for the learning-reasoning distinction comes from these facts: (a) Performance of rats in learning situations has low correlation with performance in reasoning situations (28). (b) Human I.Q. has low correlation with reasoning performance (29). (c) Operated rats learn as well but do not reason as well as normals (14). (d) Although memory for learned tasks is impaired by operation, "direction," or memory for reasoning tasks, is not affected (16).

Umweg or detour problems are said to illustrate the "reasoning" process (16, pp. 36 ff.). They are difficult not because they involve turning the back on the goal, but because they require integration of previously isolated experiences. Similarly, tool-using in itself merely tells us something about the animal's behavior repertoire. Such behavior becomes theoretically important on those occasions when it reveals the presence of a process, namely reason-

ing, different from that of ordinary simultaneous association.

(4) *Insight.* Why does Maier use "reasoning" instead of "insight" as an explanatory concept? First, while Maier believes that he has an independent operational definition for "reasoning," he can find only rather loose criteria for insight behavior. More important, however, is the fact that "insight" does not seem to refer primarily to an underlying process, but rather to a type of behavior. Maier states (21): Insight is ". . . the experience of the new relationship produced when two isolated experiences are integrated in reasoning."

Köhler (9) writes: ". . . the concept is used in a strictly descriptive fashion . . . taken in its basic sense, the term insight refers to experienced dynamics in the emotional and motivational fields no less than to experienced determination in intellectual situations." Maier believes that reasoning as he has defined and investigated it is an essential part of the principle explaining the phenomenon of intellectual insight.

CONCLUSION

1. Maier has operationally distinguished and experimentally investigated two concepts of first importance: frustration and reasoning. These refer to processes of behavior which are not uncommon, and they merit serious consideration in any treatment of motivation and learning, respectively.

2. Maier refers to other processes of behavior not too well specified: variability, preference, tension, availability, mechanism of equivalent stimuli. Some characteristics of these processes are known, but their specification is not complete. Although Maier finds these concepts useful in his own thinking, further work is necessary to settle their theoretical status.

3. Maier's methodology leads him to seek qualitative distinctions arising from various empirical laws describing behavior and (molar) neurophysiological processes underlying behavior. In several cases this program has not been very completely realized; in two important cases, reasoning and frustration, some success has been achieved.

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AN ORIENTATION FOR RESEARCH ON THINKING

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I

This paper is an exposition of an orientation around which certain research on thinking can be carried out. It is not to be considered a formal theory of thinking although, as will be seen, there are certain assumptions which clearly underlie the framework. And, even though testable problems are generated by the orientation, it will be recognized that the expected results from the tests could be accounted for largely on the basis of extant concepts. Finally, by way of introduction, it will be obvious that the orientation does not provide a framework for all recognized problems about thinking.

There is no general agreement on what problems, when presented to a subject, do or do not study behavior which can be called thinking. The behavior with which this paper is concerned is that reported in the literature under such names as *problem solving, reasoning, concept formation,* and *creativity.* For convenience, all of these modifications of behavior will be called thinking. An examination of various tasks used in these areas shows that, for solution of the task, the subject is required to learn or recognize perceptual or functional relationships among objects (or properties thereof), among symbols, or among both. By perceptual relationships are meant similarities or identities (depending upon the experimenter) in form, color, or size.[1] By a functional relationship will be meant either of two kinds of relationships: (1) a cause-effect relationship, since variation in X produces reliable variation in Y. Any known lawful relationship existing in nature is of this kind. (2) The second form of relationship is man-made, and may be thought of as mechanical. Such relationships are those obtaining between a wrench and a nut, a ball and a bat, and a horse and a cart.

It has been said above that these tasks require the subject to learn or recognize relationships. Both of the terms, *learning* and *recognition,* must be used because in some tasks the subject must discover only which one of several already known relationships is required or is appropriate for solution. That is to say, for a particular problem the subject need only recognize which one of several known relationships is pertinent. In other tasks the subject is required to learn relationships which during the normal course of his life he would never have acquired. Thus, if the task requires the subject to show that automobiles, horses, and trains are related (all are methods of transportation), it would be the recognition of a relationship. But, the subject might be required to learn that all blue triangles represent the concept DIT, while all red triangles represent REC. Such relationships would not normally have been acquired in our culture. These are man-made relationships for purposes of research only.

It is recognized, of course, that thinking may also require the recognition or learning of relationships among relationships. Such problems approximate what is sometimes referred to as creative thinking (8), and are, descriptively, more complex than the tasks described above. Several of Maier's prob-

[1] Attributes of stimuli in other sense modalities could likewise be specified.

lems (15) meet these requirements as do some of those given by Duncker (5) and Wertheimer (29). Syllogisms are problems in which two relationships are given and the subject is to decide what third relationship is based on the two given. Yet, the basic requirement is still that of perceiving relationships.

II

Behavior changes called thinking meet all the definitional requirements of learning. This does not mean, of course, that the inferred processes which are said to produce behavior changes (also called learning) resulting from conditioning techniques are the same as those which produce the behavior changes called thinking. Nor does it mean that there is *not* a high degree of communality among these processes. It will be the working assumption here that there is communality among the processes producing all behavior changes which fit the definitional requirements of learning. While such an assumption is not essential for the development of the orientation it will be seen that it is implied and may therefore be stated flatly. It is the sense of the present paper that the greatest need at present for understanding thinking is a set of empirically well-established laws between stimulus dimensions and response variables. Such laws might destroy some of the chronic problems which show themselves in arguments of insight vs. trial and error, single vs. multiple learning processes, learning by understanding vs. learning by rote, etc. On the basis of present data it is impossible to give convincing arguments concerning the reality or irreality of these demarcations; the data on thinking at the human level are far too meager. But, we may ask: What data are needed to help dissipate some of the disagreements?

There are at least three ways by which we might focus more definite answers to questions concerning the amount of overlapping of basic processes responsible for the wide variety of behavior changes from those called conditioning through those called thinking. One of these methods is that of correlation analysis. If a subject does well on a rote learning task and poorly on a thinking task, there would be some basis for inferring the lack of similarity among the processes needed to perform the two tasks. No serious attempts have been made with this approach. Maier (16) reports some data on rats which suggest low communality among various processes. Billings (1) shows a moderate relationship between what might be called rote learning and thinking. Nevertheless, it is a fair guess that the results of even a thoroughgoing factor analysis of behavior on a wide variety of tasks, from conditioning through thinking, would demand the postulation of many different processes if the lack of inter-correlation is the basis for such postulation. It is known, for example, that in the area of motor behavior, where apparently simple skills *seem* to be highly similar, correlations may be quite low (23). Thus, if we require the postulation of different processes whenever correlations are low, we will likely have scores of processes. This may be necessary, but it is no premise on which to proceed at the moment. Correlations may be low for a variety of reasons; they may reflect operation of secondary factors or processes which are masking the influence of processes which in effect are common to the performance of a wide variety of tasks. Or, all processes may be common to all learning but the amount of each required may vary widely. In short, factor analysis would probably lead to the postulation of a

number of processes which might not actually be required by theory.

A second method of unifying data at various levels of learning is in terms of theory directly. If theoretical concepts derived from one area of discourse are shown to be capable of accounting for phenomena at another level, one may feel some confidence in bringing the two areas together. Some progress in this direction has been made with conditioning and rote learning (7, 11). It cannot be given an adequate tryout with, say, thinking and rote learning, because of the paucity of data on thinking. Certain attempts to relate concept formation and less complex forms of learning have not been without some success (7, 27), but the lack of systematic laws about concept formation does not allow a definitive evaluative attempt.

The third method is that of determining the functional relationships between manipulable variables and behavior for various levels of behavior which meet the definitional requirements of learning. If these lawful relationships turn out to be similar for an appreciable number of independent variables then again we have a strong basis for inferring common processes.

Actually, of course, there is no incompatibility between the second and third approach; they should be carried out simultaneously. A program of research based on the third approach could proceed without theory, but it is my belief that in the long run it would become sterile. However, it should be stated again that the ultimate fruitfulness of the second approach (theory) cannot be determined until some empirical laws (third approach) are obtained against which the theory can be checked, verified, modified, thrown out, etc. The conclusion is that a great deal of the future development in thinking rests squarely on filling the hiatus representing lack of empirical relationships between stimulus variables and response changes for tasks said to be measuring thinking. The purpose of the present orientation is to set up simple hypotheses about the influence which some of the environmental and task variables should have on rate or efficiency of thinking.

III

In this section the basic assumption on which this orientation rests will be stated. In behavior called thinking the subject is motivated to solve a problem—reach a goal—and the solution depends upon his learning or recognizing certain relationships among symbols, objects, or among relationships themselves (as discussed above). For simplicity of exposition, in future references these symbols, objects, and relationships will all be called stimuli. *Stimuli,* therefore, will be used in a general sense to mean any of the following: (1) objects *per se;* (2) physical properties of those objects; (3) symbols which may stand for a functional relationship, for an object, or for a property thereof; (4) functional relationships which become cues by virtue of being responses to objects, properties of those objects, or symbols; [2] and at times (5) memorial representation of any of these stimuli.

For the perception of relationships among stimuli the needed assumption is that the *appropriate responses to those stimuli be contiguous.* By appropriate is meant the perceptual, ideational, or motor response which defines the stimulus property demanded by the

[2] Response-produced stimuli have been discussed in detail by Miller and Dollard (18) and Dollard and Miller (4). It is their belief that stimuli-producing responses are largely verbal (4, p. 101). It should also be clear that these events may extend into a series of relatively implicit stimulus-response units.

relationship. To illustrate with a simple case: If a concept to be evolved is "blueness," and the subject is presented with blue triangles and blue circles, the common property can be perceived only if the two responses "blue" occur contiguously.[3]

The orientation may be made more clear if a mechanical model is used. Assume that there are six capsules in a bowl, each capsule representing a single stimulus. The solution to the problem requires that the relationship among three of the six stimuli be perceived. Assume further that the subject is an automaton set to draw three capsules at a time. After each draw the capsules are returned to the bowl. When the three appropriate capsules are drawn simultaneously, *when they appear contiguously*, the problem is solved. For such a situation the time of solution can be calculated on a straight probability basis. On a single draw of three capsules the probabilities are .05 that the correct three will be drawn. ($n^cr = \frac{n}{(n-r!)(r!)}$, where n^cr is the number of combinations of n things taken r at a time.) There are two essential points to be made by this mechanical analogy. (1) Drawing three at a time is the mechanical analogy of achieving maximum contiguity of responses to stimuli. Thus, if the appropriate concept is "blue" and the subject responds spontaneously with such a response to the three stimuli in rapid order, we would be achieving maximum contiguity. (2) The second point is that human subjects are *not* automatons set to draw on a random basis. The human subject has a memory by which contiguity of responses may be produced even though the physical stimuli for those responses are not present. Furthermore, the human subject need not draw three stimuli at a time. He brings past experiences to the situation which might allow him to discard one or more of the stimuli as irrelevant before he starts drawing. But, because problem solvers are human does not necessarily mean that they would be superior to the automaton at solving problems. For they have biases and sets and prejudices which may retard as well as enhance solutions. Thus, the problem of manipulable variables in studying thinking might be thought of as the study of factors which enhance or decrease probabilities of solution over those offered by a straight mechanical analogy. The main problem, then, is to suggest factors which are likely to increase or decrease the probabilities that pertinent responses will become contiguous.[4] This will be accomplished in the following section.

[3] No new assumptions are needed concerning matters of reinforcement. In rote learning it is known (25) that the anticipation of the correct response to a given stimulus strengthens the stimulus response connection, i.e., increases the probability that the stimulus will elicit the response. It shall be assumed that the same process operates in thinking. When appropriate responses occur contiguously (and are in some way known to be correct) there is an increase in the probability that the stimuli will elicit the response.

IV

Contiguity. The basic assumption is that in order for relationships among stimuli to be perceived and acquired, responses to those stimuli must be contiguous. As with most basic assumptions, there is no way to make a direct test of the validity of this one. But, conditions may be set up in which temporal contiguity among relevant stimuli is varied and thereby show an inverse relationship between actual time among

[4] It is quite possible, of course, that pertinent stimuli may be contiguous without the pertinent responses to those stimuli being made or being contiguous. This is largely a problem of individual differences.

these stimuli and rate of solution. It is assumed that contiguity operates in the manner specified because the greater the interval between pertinent stimuli the greater the subject must rely on memory; hence, the less the probability that pertinent responses will be contiguous.

As an illustration, let us consider a concept-learning situation. Twelve stimulus objects of some complexity are presented to the subject in a series. Four of the objects represent one concept, four another, and the other four still another. Each stimulus is presented for, say, five seconds for the subject to study, and then the subject is told the response which goes with that particular concept. The subject must, in each case, determine what the relevant stimulus dimensions of the various objects are. According to the mechanical analogy, if all four stimuli of a set were placed in front of the subject, and if he put a capsule for each dimension of all stimuli in the bowl, successive drawings would allow him to discover the common dimension running through all four. But, in serial presentation he cannot do this. He must "draw" by (ideally) stating hypotheses and testing them on each item until rejected or confirmed. To reject or confirm an hypothesis at any given moment, he must rely on his memory for the dimensions of the objects not physically present. Because of the fallibility of memory we would expect that the greater the time between pertinent stimuli the slower the rate of acquiring a concept. Assuming that retention is inversely related to time, putting the four stimulus objects in close temporal contiguity should enhance solution; separating them by other stimulus objects (irrelevant) should retard solution.

There has been no adequate test of this hypothesis. Gagné (6) has shown that temporal closeness of highly similar stimuli leads to more rapid discrimination in rote learning, but where thinking as described in this paper is involved, no data are available. The test proposed above would not be entirely unambiguous, for by varying temporal contiguity of stimuli one is also varying the degree of massing and distribution of those stimuli. Furthermore, if the concepts appeared in close contiguity the subject could use some abilities of logical elimination which he could not use if the samples of a set were randomly distributed among samples of other sets. It is possible, however, to overcome both of these difficulties either by design or by interpretation (see later discussion).

In summary, it should be reiterated that insofar as can be told at present, there is no way by which we can test the proposition that contiguity of pertinent responses is necessary for solution.[5] But it does seem possible to demonstrate that time separating stimuli to which the pertinent responses must be made (for solution) would be an important variable influencing thinking.

Perceptual vs. symbolic presentation of stimuli. These two terms, perceptual and symbolic, may not carry the exact meanings intended. However, an illustration should clarify the distinction. Presenting an object or a picture of an object will here be called a perceptual presentation of a stimulus; presenting the object name will be called a symbolic presentation. Thus, a picture of a fish is perceptual presentation; presenting the word "fish," a symbolic presentation.[6]

[5] Maier (16) hints strongly that contiguity in experience is not essential for problem solution. Obviously, this idea is rejected in the present formulation.

[6] No clear-cut dichotomy is possible. The barest outline of a fish may be equivalent (in our predictions) to the word fish. The prob-

In dealing with this manipulable variable, the basis for prediction again concerns the fallibility and incompleteness of memory. However, no overall prediction can be made concerning rate of solution. We shall be concerned with two particular situations which will indicate the factors which need to be considered in making predictions.

1. Properties (stimuli) of objects may not be remembered when the stimuli are symbolic, and if these properties are germane to the solution of a given problem, solution will not be achieved. To return again to the mechanical analogy, failure of memory would mean that some of the pertinent capsules would not be in the bowl. To illustrate: If we ask a child what relationship exists between a table and a sheep, it is likely that the common four-leg property will be discovered more readily if the sheep and table are presented perceptually (real or by pictures) than if the child must depend upon memory as he searches for common properties.

2. On the other hand, if the forgotten properties of the symbolized object are irrelevant and the remembered properties relevant, solution might be more rapid for symbolic presentation than for perceptual presentation. For, in the case of perceptually-presented objects, properties which were forgotten for the symbolized objects may actually serve as potent distracting stimuli. In the mechanical analogy, all pertinent capsules are in the bowl but fewer irrelevant capsules are present when the symbol is presented than when the object itself is presented.

The above considerations make it clear that in order to derive unambiguous predictions concerning problem solution for perceptual vs. symbolic presentation of stimuli, it is necessary to know the location of the pertinent response or responses in the hierarchy of responses to the two kinds of stimuli. To bring this variable under adequate experimental control, two approaches will be mentioned.

The first approach provides a limitation on the number of stimuli. Thus, we might use simple stimuli for which the number of possible responses is very limited. Simple geometric forms provide such materials. For complex stimuli, a limitation on the number of dimensions to be considered by the subject could be afforded by proper instruction or pretraining. For example, we might have the subject first learn that this object (for which a symbol stands) has, say, ten characteristics, one of which is pertinent to solution. We could likewise point out to subjects in another group that this perceptually-presented object has the same ten characteristics and that one of these is pertinent to solution. It would then be predicted that in an actual problem-solving situation the perceptual stimuli would produce more rapid solution since the ten characteristics of the object indicated by the symbol would be less available because of forgetting.[7]

A second approach requires first that materials be calibrated. We would need to know, for example, the dominant responses to a perceptual stimulus and to the corresponding symbolic stimulus. For example, if a photograph of a fish is presented to the subject and he is asked to "tell all you can about a fish," how will the responses compare with those when he is merely presented the word "fish" and given the same instructions? Knowing how the response

lem is obviously amenable to experimental attack. In the present paper, however, we will be concerned only with the extremes of the perceptual-symbolic dimension.

[7] It is possible that the speed with which perceptual stimuli can be "drawn" and "rejected" is greater than with recall "drawing." If this is true, predictions will have to be modified accordingly.

hierarchies are arranged in the two cases, we could choose the pertinent (correct) characteristics for concept formation studies which have greater, less, or equal probabilities of being elicited by either method of presentation, and predictions of solution rates made accordingly.

This particular variable cannot be manipulated for problems which require the perception of relationships among relationships for solution. That is, it cannot be varied unless it be said that graphical or algebraic representation is perceptual representation of a functional relationship.

There is some experimental evidence to support the expected relationship between efficiency in problem solution and perceptual representation of pertinent stimuli. Maier (17) has shown that if a model is set up to solve one problem, and this model possesses properties essential for solving a subsequent problem, solution will be more rapid if the model remains in the immediate perceptual field. This is not a clean-cut verification of the proposition, however, since the presence of the mechanical model may have tended to limit the subject's search more than if the model were not present. But it may mean that certain properties or functional relationships suggested by the model were less available on a recall basis than on a perceptual basis.

Number of stimuli. The greater the number of stimuli involved the slower will be the rate of solution. The stimuli may be relevant, irrelevant, or both, and the proposition still holds.[8] For, with an increase in the number of stimuli goes a decrease in the probabilities that the pertinent responses to stimuli will become contiguous. The greater the number of capsules we put in the bowl the less the probabilities that the pertinent ones will be drawn simultaneously so that the responses to them are contiguous. Confirmation of this principle may be accepted tentatively from the results of a study by Reed (22). In this study the appropriate concept word was placed among four irrelevant words in one condition and among six irrelevant words in a second condition. Concept formation was more rapid in the first instance.

This proposition has a great deal of face validity and is probably not one which requires urgent experimental work.

Similarity among stimuli. No categorical prediction can be made concerning the influence of similarity among stimuli on rate of thinking. It must first be recognized that the similarity which is often dealt with in thinking is learned similarity, and thus is itself a relationship. Physical similarity, as among tones, colors, sizes, have sometimes entered into stimuli used in concept studies and does, of course, provide a basis for careful quantification of similarity dimensions.

The influence of similarity can be most carefully analyzed in concept-formation studies. Welch (28, p. 236) has indicated two basic relationships which appear adequate for this situation. If, in the concept formation study, the similarity obtains either among the positive (relevant) stimulus dimensions, or among the negative (irrelevant), concept formation will be most rapid. If the similarity holds both among the positive and negative instances, solution will be slow. Welch also presents some confirming evidence (28, p. 225 f.).

Gibson's theory (7) of verbal learning, which she also indicates may handle concept formation (at least certain

[8] One of my students, Ross L. Morgan, has pointed out that it may be necessary in future developments not to speak merely of number of stimuli, but of the ratio between the number of relevant and irrelevant stimuli.

aspects of it), stresses generalization as the process which would account for the influence of similarity in concept-formation studies. Recent studies by Buss (3) and Oseas (21) have shown that it is possible to derive gradients of stimulus generalization from the errors made in learning concepts. Thus, on at least this one basic explanatory concept, we do have a rapprochement of conditioning, rote learning, and tasks which fall within the category of thinking.

We have said that similarity may retard or enhance thinking depending upon the particular stimuli among which the similarity obtains. From analyses of transfer in verbal learning it is known that if one response is to be attached to two similar stimuli, high positive transfer will be measured; if a different response must be made to these two stimuli, negative transfer will occur. It can be seen that this is essentially the situation we have in concept learning as described above. From another point of view, it can be shown that among verbal stimuli at least, similarity (scaled synonymity) is almost perfectly correlated with associative connection (as defined also by scaling). Haagen's work (9) shows this clearly. Thus similarity acts as a biasing agent so that in the mechanical model certain capsules have a greater probability of being drawn than do others. More specifically, if the capsules were of different size with the large ones representing similar stimuli, the large ones would have the highest probability of being drawn. Responses to these stimuli thus have a high probability of occurring contiguously. If a common response is to be made to these similar stimuli, facilitation will occur; if different responses, inhibition will occur.

In more complex thinking in which solution demands the perception of relationships among relationships there is every reason to believe that the above principles of similarity obtain. Syllogistic reasoning would fall into this category as would analogistic reasoning. Anyone who has taken some of the more difficult analogies tests can be easily convinced of the major role played by similarity. All factors considered, it would appear that we can have high confidence in the importance of similarity as a variable, and, furthermore, that we know fairly well the direction of its influence in various situations.

Biases. It was mentioned previously that one of the major factors which prevents the mechanical analogy from being accurate is that living organisms have memories. A second major factor which further prevents the direct analogy is that subjects have response biases. It has been shown that the influence of similarity might be deduced by considering the biasing effect which it has. Any bias produces a selection of responses to stimuli, or in some instances may select stimuli; thus all stimuli (and all possible responses to a particular stimulus) do not have equal probabilities of being drawn. And it is patent that these biases may enhance or retard thinking. If the subject is biased toward irrelevant stimuli, rate of solution will be slow; if biased toward relevant stimuli, rate of solution will be fast. Furthermore, the subject may cast aside capsules on a rational basis; that is to say, he may believe that some capsules are irrelevant to solution. This process is in itself thinking and should, according to the present orientation, be subject to the same laws as the solution toward which the elimination of capsules was directed.

Over a period of years the literature has developed a rather sizable catalogue of stimulus-response biases. For the present we need only list some of the more prominent ones.

1. Woodworth and Sells (30) have demonstrated clearly that in syllogistic reasoning the negative or positive atmosphere of the premises produces response bias. This "atmosphere effect" tends to produce negative conclusions if the premises are negative and positive conclusions if the premises are positive. Prejudices will also tend to cloud straightforward solution of syllogisms when the premises contain ideas which tend to elicit emotional responses. This has been shown clearly by Morton (20), Thistlethwaite (24), and Lefford (12).

2. Heidbreder's long series of studies (10) have shown consistently that in concept formation we tend to respond first on the basis of objects, then form, then number. Thus, an object such as a house will be more quickly categorized than will a wreath representing a circle.

3. Luchins, in his studies of set or *Einstellung* (13, 14), has shown the great potency which past success in solving problems in a given fashion has on the solution of subsequent problems. In line with this, Maier (15) presents some evidence that one of the primary causes of failure is that the subject persists in trying solutions of a very restricted nature. Explicit instructions to vary attacks on problems tends to decrease routine and unsuccessful attempts.

4. Morgan (19) has demonstrated that in a situation where several stimulus properties have equal logical priority as critical properties, size and position will predominate over form and pattern.

5. The many observations made by the Gestalt psychologists demonstrate that the initial stimuli attended to in the visual field are by no means on a chance basis. Closure, good figures, etc., all tend to bias perceptions, thus voiding the possibility that all stimuli have the same probability of being responded to, i.e., of being drawn from the bowl. And we are just beginning to realize that motivation may have a selective influence on responses.

The above is not an attempt to list all biases; they have been given as illustrations of the basic fact with which we must deal, namely, that all stimuli in a given situation do not have equal probabilities of being responded to. It is believed that the study of these biases is an extremely important one for obtaining an overall set of principles which will determine efficiency in thinking. Indeed, these biases are in themselves so important that we may expect miniature theories to develop around them. A great many studies will be needed to determine the influence of manipulable variables on these biases.

V

In this section mention will be made of the more obvious problems associated with the study of thinking which are not considered in the present orientation.

First, it is recognized that there are logical tools used in thinking which commonly develop in all people in our culture. These tools are those which are necessary in order to discriminate among stimuli. Relationships such as big-little, up-down, under-over, etc. are necessary knowledge if differences among stimuli are to be responded to. So also are the tools which lead us to conclude that if A is greater than B and B greater than C, then A is greater than C. Which tools or methods are going to be most successful in solving problems will certainly be important variables to investigate, but the present orientation develops no specific prediction concerning these matters. It is clear, however, that a particular method

is in effect a biasing agent and whether or not it will be successful depends upon the nature of the task. Some evidence is available which supports this contention (2). A specific method restricts the range of responses, and, depending upon the problem, may facilitate or inhibit solution.

Does the orientation outlined here imply that the best way to solve problems is to put all seemingly relevant stimuli into a bowl and keep drawing and drawing different combinations of stimuli, each time searching for common properties? It would appear that such a procedure would have some advantages in that it should eliminate many biases. And it is possible that many new relationships might emerge from such a procedure. It is obvious also that such a recommendation removes all vestige of the psychic from thinking. But the method has certain evident limitations. If all relevant stimuli are not in the bowl, solution to the particular problem would be impossible, although unexpected relationships might emerge. The orientation outlined here requires that pertinent responses to stimuli become contiguous. Therefore, even though the appropriate stimuli are drawn contiguously, there is no assurance that the appropriate responses to those stimuli will be made, that is to say, appropriate in the sense that they would provide solution. Why one subject may respond with the appropriate response and another may not is a problem—and an important one—not handled in the present paper. It is a fair possibility, however, that these differences in behavior represent in part differences in the magnitude of influence of the variables here said to be important for subjects at large.

Another matter of importance is the influence of practice in thinking on thinking. We know that in less complicated forms of learning, practice effects may be very large. We need likewise to know if the practice effect in thinking is large or small. No systematic data are available on this matter at the present time.

There are also problems related to productive thinking (29). Particular problems can be solved without the subject also acquiring a general principle by which other problems can be solved. The present orientation makes no prediction concerning this matter; that is to say, no prediction is made concerning the transfer effects following solution. It is clear, however, that many relationships are useful in solving other problems regardless of whether that relationship is understood or learned by rote. There is no reason to disbelieve the general idea that learning with understanding will lead to more adaptive behavior in other situations; likewise, there is no reason to doubt the usefulness of rote-learned relationships for thinking.

The basic assumption of response contiguity presented here may be interpreted to give a prediction that spaced practice would be inferior to massed practice in thinking. Spaced practice would reduce the likelihood of response contiguity because of forgetting which takes place during the spacing interval. And even though spaced practice is usually defined in terms of time between trials and not the time between stimuli representing the same concept, the prediction should still hold. The published data on massing vs. distribution in thinking tend to show that massing does result in better performance. Such data support the basic assumption of the present paper. It has been maintained (26, p. 439), however, that these studies have not adequately explored the time dimension of distribution and that no inclu-

sive generalizations are possible. A recent study on concept formation (21) has shown that distributed practice up to one minute between trials produced slightly superior performance to the massing of trials. In short, it is possible that for certain kinds of tasks involving thinking, e.g., concept formation, where stimuli may be presented fairly rapidly, other processes may operate to such an extent as to obscure the differences produced by variation in response contiguity *per se*. All evidence of rote learning points to the fact that distribution of practice will facilitate only when the subject is required to respond rapidly. There is no reason to believe that the factor or factors which operate in these cases will not also operate in thinking if the speed is comparable to that of rote learning.

Other variables, such as subject variables (sex, age, intelligence) are also ignored and will need to be evaluated before any theory of thinking reaches a point that could be called comprehensive. Before such a theory can be developed, many empirical laws must be determined. The present framework is to be thought of as a device for directing the research for obtaining some of these laws.

VI

Summary. A point of view concerning thinking was developed for the sole purpose of giving direction to research on certain variables which appear to influence efficiency in thinking. The one essential assumption made was that for new relationships to be acquired the pertinent responses to stimuli must be contiguous. With this assumption, predictions concerning the influence of certain manipulable variables on thinking were made. Some admittedly important problems are not handled by the orientation as it now stands.

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INCIDENTAL MEMORY AND PROBLEM-SOLVING *

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A number of experiments on learning and problem-solving seem to indicate that memory may sometimes prove to be a hindrance in efficient problem-solving. Among such experiments might be cited those of Duncker (2), Luchins (7), Krechevsky and Honzik (5), and Maier (10).

Duncker, in his monograph on problem-solving, adduces evidence that one use of an object for the solution of a given problem may hinder the solution of another problem in which the same object must be used in another function. The inhibitory effect of the previous functional use of an object upon its use in other problems is called by Duncker "functional fixedness." Luchins (7) in his "standard exploratory experiment" has shown that the use of the same problem-solving procedure in a succession of similar problems tended to blind the subjects to other (and more efficient) possible solutions in later problems. Krechevsky and Honzik (5), working with rats, investigated the effect of overlearning upon the plasticity of a learned and originally "docile" response. They found that excessive overlearning tended to fixate behavior so that it could not be readily changed in response to a changed situation. Finally, Maier (10) in connection with his experiments on problem-solving and reasoning developed the concept of "habitual direction" by which he referred to the tendency for individuals to reproduce old solutions in new problem-situations. To demonstrate that habitual directions may be an obstacle to the solution of the problem, Maier gave one of his groups a short lecture on problem-solving in which he discussed the effects of such "habitual directions." Maier found that such instruction resulted in an improvement in problem-solving behavior as compared with an uninstructed group.

As I have already suggested, the above findings can be interpreted as indicating the inhibitory effect of memory upon problem-solving. In the first three cases remembered past solutions seem to inhibit new approaches to new problems. In the fourth case, the instructions given in the short lecture may be said to have counteracted the effect of memory.

Now, if it is true that memory may hinder the solution of new problems, one might expect that individuals who excelled in certain memory abilities would be low in problem-solving ability.

It will be understood that not all kinds of memory could be expected to have an inverse relationship to problem-solving. As Köhler (4) and Katona (3) have shown, items that are organized into wholes are more easily reproduced than more isolated items. Memory evidently depends upon organiza-

* This study would probably never have been carried out if it had not been for help and encouragement from Professor David Krech. His theoretical thinking clarified the concepts and made possible an experimental set-up. The data seem to be in line with what could be expected from Krech's general psychological theory as described in this JOURNAL, November, 1950. I take the opportunity to thank David Krech for his inspiring teaching at the University of Oslo during the academic year 1949–50. I also want to express my gratitude to Mr. Erik Rinde and the Norwegian Institute of Social Sciences for the economic support which made the experiment possible.

tion, i.e., to some extent probably upon the same factors that influence problem-solving. Hence the hypothesis must be formulated more specifically as follows: *A negative correlation should exist between the ability to remember isolated items and the ability to solve new, difficult problems.*

As many psychologists have underlined, isolated items will probably never exist to an individual. All items will probably be interpreted in some way and thus to some extent enter into some kind of organization. When we speak of isolated items, it will be understood that we refer to items that we would expect would be hard to organize into some meaningful whole.

As for the problems, it will be seen that it is important that the problems are new to the subject in the sense that the elements necessary for the solution have to be combined in a new way. If this is not so, a good memory may be an advantage in attempts at solving the problems.

It will be seen that the hypothesis will be in line with the assumption that rigidity and concreteness go together (12). Rokeach found a correlation of − .19 between Einstellung-effect and intelligence (Stanford-Binet a). The reason this negative correlation was not higher may be due to the fact that an intelligence test may measure both memory and problem-solving. A person high in memory and low in problem-solving may obtain the same score as one low in memory and high in problem-solving.

It might perhaps also be pointed out that the hypothesis advanced need not be in opposition to the findings that the fast learners are the best retainers. Learning and retention may be continuous processes (11, p. 388). If, for example, learning is favored by good memory, the good retainers will probably also be the best learners.

Procedure

The memory test. Memory for isolated items was measured by an incidental memory test. A rote learning test with nonsense syllables was also tried, but was soon dropped. In rote learning you cannot control the effect of the instructions given (3, 4). Instructions will probably stimulate some sort of organization of the material.

Two pictures, each showing a group of persons, were used as stimulus material. The first picture had a background that partly resembled a sitting room and partly an office. The other picture was a street scene. Care was taken that neither picture had an obvious overall central theme that would allow organization under some general idea.

The test was given as a group test. The subjects were told they would be given a memory test. They were instructed to look at only the clothing of the persons and told that they would be asked to give a description of the clothing afterwards.

To assure that the subjects had seen the background, each picture was shown for a period as long as 2½ minutes. The pictures were presented in succession. After the presentation, the subjects were first asked to give a description of the clothing, and, when that was completed, they were asked to describe the background. A questionnaire regarding the background was also filled out. The questionnaire contained a lot of dummy questions partly to warn the subjects against guessing and partly to allow a check on whether or not the subjects guessed. Care was taken in the instructions that the subjects would not anticipate being asked about the background. In scoring, one

point was given for each object remembered correctly and one-half point for each additional characteristic of the object mentioned. The subjects were asked to describe the pictures in as great detail as possible.

One might argue that this kind of test, besides measuring memory, also measures concentration. The subjects with a high ability to concentrate on the clothing of the persons in the pictures would not remember as much of the background as the subjects with a lower degree of concentration. If an inverse correlation between incidental memory and problem-solving was obtained, this might be explained by the fact that those high in concentration would be low in incidental memory and high in problem-solving.

This interpretation can, however, be checked by the description of the clothing of the persons. If concentration is the main factor in the incidental memory test, one would expect a negative correlation between memory for the clothing and memory for the background.

The Problems

The following five problems were used:

1. A slightly modified version of one of Maier's (8) problems. A lighted candle is to be put out from a distance of about 2½ m. The following objects placed on the table could be used: a wooden stick, 135 cm. long and about ½ cm. in diameter; eight tubes of glass, 29 cm. long and 1 cm. in diameter; a saucer with a lump of putty; and a jug of water. The candle should not be turned upside down. The time limit is ten minutes.

The correct solution is to join the tubes together with putty to form a long tube, put the stick into the tubes to hold them stiff, and then to blow the candle out through the tube.

2. A match problem taken from Katona (3). The subjects were presented with five contiguous equal squares and asked to form four squares out of these five. The new squares must all be the same size. Two matches cannot be placed beside each other to form one side. The squares must hang together at least in one corner. Only three matches may be moved; these matches must be used to form squares; they cannot be laid aside. The time limit is ten minutes.

3. An iron container 24 cm. high and 5 cm. in diameter is placed on a table. A piece of wood shaped like a cylinder is dropped down in the container. Any of the following objects placed on the table may be used: a pair of pliers, a penholder, a piece of thread, a screw driver, a tray with glasses, and a jug of water. The tray with glasses and the jug of water was always on the table in the lecture room in which the problem was set up.

The subjects were instructed to raise the piece of wood without turning the container, and allowed ten minutes. The correct solution was to pour water into the container.

4. The problem is to form four equilateral triangles with six matches, the length of the sides being that of a match.

If the subjects knew the problem, they were given, as an alternative problem, the "13 problem" from Duncker (2, p. 31). All six-place numbers of the type 276, 276 or 385, 385 are divisible by 13. Can you tell me why? A common solution is to show that any number of the type abc, abc is a multiple of 1001, which is divisible by 13. The time given is ten minutes.

5. The problem with the horse and rider from M. Scheerer, K. Goldstein,

and E. G. Boring (13). The drawings in Boring, Langfeld, and Weld (1) were shown, and the following instructions given. "Here are two figures. By placing this (the small one) over that (the big one), you may get this result," pointing to the figure on the right side of the page. Time given was ten minutes.

The first time the experiment was performed, the candle problem was not used. Instead, the subjects were given Maier's (8) string problem. That was dropped, as it seemed to favor the boys. One of Gottschaldt's figures (2, p. 106) and the figure with the number 4 in Köhler (4), were found to be too easy, as the scores did not discriminate between the subjects. Duncker's radiation problem was also dropped as that seemed to be too difficult and also seemed to favor persons trained in physics.

The problems were not given to the subjects in any fixed order. The subjects were tested individually on each problem and were told not to speak to their friends about the problem. The score was the number of correct solutions for each subject.

Subjects

Fourth-year students from Oslo high schools served as subjects. The average age was about eighteen years. The experiment was first performed with eighteen subjects, eleven boys and seven girls. As no relationship of the expected kind appeared, it was decided to start the experiment again with boys and girls in separate groups. The experiment was repeated with forty-nine boys and twenty-one girls. The subjects had had the same curriculum to within two years before the experiment. During those two years thirty-six of the boys and six of the girls had mathematics and physics as their major program of studies, whereas thirteen boys and fifteen girls had specialized in languages.

The memory test and the problems were altered slightly during the second administration. The results presented are, therefore, those obtained from the second experiment.

Results

The following product-moment correlations were computed:

1. For incidental memory and problem-solving for the forty-nine boys, $r = -.46$, significant at the 1% level. (Means: 6.3 and 2.4; σ's: 2.8 and 1.3.)

2. For incidental memory and memory for the persons' clothing, $r = +.27$. (Mean and σ for memory of clothing: 38 and 8.)

3. For incidental memory and problem-solving for thirty-six boys with mathematics and physics as main subjects, $r = -.38$, significant at the 5% level. (Means: 5.9 and 2.6; σ's: 3 and 1.2.)

4. For incidental memory and problem-solving for the thirteen boys with languages as main subject, $r = -.60$, significant at the 5% level. (Means: 7.3 and 1.5; σ's: 2.0 and 1.1.)

5. For incidental memory and problem-solving for the twenty-one girls, $r = -.10$. (Means: 6:5 and 1.1 and σ's, 2.1 and 0.9.)[1]

[1] I.Q.'s for the subjects were not readily available; instead school grades were examined. Grades were obtained for 37 subjects. For 24 boys (group I) with mathematics and physics as main program of training, the following four grades were taken: (1) a general grade—all school grades summed; (2) mathematics; (3) physics; (4) Norwegian composition. For the 13 boys (group II) with languages as main program of training, the following four grades were used: (1) a general grade; (2) mathematics; (3) principal language; (4) Norwegian composition. The Norwegian schools ordinarily use a scale consisting of six steps.

Discussion of Results

These r's, quoted under (1), (3), and (4) of "Results" indicate that a relation of the kind stated in the hypothesis probably exists.

The explanation of the relation as being due to different degrees of concentration in the subjects high and low in incidental memory seems to be ruled out by the relatively high positive r between the two kinds of memory. According to this explanation, one might have expected a negative correlation between incidental memory and problem-solving. Upon further thought, the hypothesis that a high degree of problem-solving should go with a high degree of concentration is not very probable. It is more likely that a high degree of concentration may be a hindrance to problem-solving.

The $r = +.27$ for incidental memory and the description of the clothing might indicate that this kind of memory is also negatively correlated with problem-solving. However, the correlation between these two variables gave an $r = +.001$. Further investigation of these two kinds of memory should prove interesting.

A study of the scores on problem-solving for the boys with mathematics and physics as main subjects and those with language as the main subject shows a difference of 1.10 (the mean score for the first group is 1.64 and for the second 2.54). A computation of Fisher's t-test gives a $t = 2.82$, which is significant at the one per cent level. This difference may be interpreted in two ways. The difference may be due to the fact that the good problem-solvers choose a training in mathematics and physics, or it may indicate the effect of training in mathematics and physics on problem-solving. If the latter is true, one might perhaps argue that the difference in training of the forty-nine subjects may have influenced the relationship between memory and problem-solving for the forty-nine boys in such a way that those with language as their main subject are trained in memory [2] and those with physics and mathematics are trained in problem solving. A computation of r's for the two groups separated shows that this is probably not true.

Regarding the low negative correlation, when the boys and girls were regarded as one group, it should, in the first place, be noted that the girls solve fewer problems than the boys, the

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The above-mentioned grades were correlated with incidental memory and the following r's obtained:

For group I:

(1) $r = -.45$; (2) $r = -.52$;
(3) $r = -.47$; (4) $r = -.0002$.

For group II:

(1) $r = -.61$; (2) $r = -.35$;
(3) $r = -.08$; (4) $r = -.46$.

Of these r's the first three for group I are significant, respectively, at the 5%, 1%, and 5% level. For group II the first r is significant at the 5% level (Fisher). An r for mathematics and problem-solving was obtained for both groups: $r = -.01$ and $r = -.15$.

The results seem to indicate that further research on these relationships is worth while. The significant negative correlations between incidental memory and mathematics and physics seem to confirm the hypothesis of a negative correlation between memory for isolated items and problem-solving. The lack of positive correlation between mathematics and problem solving appears to be a little surprising. The lack of positive correlation may possibly be explained by the fact that mathematics, at a relatively low level, may be mastered by persons with good memory without deeper insight into the relationships.

[2] As will be seen, there is a difference in the means of the scores for incidental memory for these two groups. The difference does, however, not reach significance.

means being 2.34 for the boys and 1.01 for the girls. A computation of Fisher's t gives a $t = 14.10$, which is significant at the one per cent level.

This indicates that there probably is a difference between boys and girls in problem-solving ability. L. L. Thurstone, in reading this manuscript, has pointed out that at least four of the five problems involve some visual space factors, factors in which he is inclined to believe there are sex differences (**14**). If this should be true, the lack of a significant negative correlation for the girls still remains to be explained. In this regard, it should be pointed out that the number of problems solved by the girls constitute a very limited range. The scores cluster around 0 and 1 (15 of the 21 scores). This may explain the lack of a significant negative correlation. A further discussion of this point should, however, be postponed until more experimental data are available.

Summary

Previous experiments suggested that memory may prove to be a hindrance in efficient problem-solving. To test this hypothesis, forty-nine male and twenty-one female high school students were tested on incidental memory and on problem-solving. The scores for the two kinds of tests gave a negative correlation, significant at the one per cent level for the male students, whereas no significant negative correlation was found for the female students.

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EFFECT, CHANGE, AND EXPECTATION OF REWARD

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The present paper constitutes an attempt at theory construction using the intervening variable technique. Our purpose is to present a theory which, while violating no established principle of Hullian analysis, will explain certain results which do not follow from Hull's postulates. Our procedure will be (1) to present the theory in the first two sections, and (2) to derive the crucial results in the next, and last, two sections.

I. Growth of Excitatory Potential

In Hullian theory, as represented by the 1949 Revision of the Postulates (**3**), the incentive function K is supposed to remain constant throughout a series of trials so long as the reward in each trial remains constant. The growth of excitatory potential ${}_sE_R$ depends solely upon the growth of habit strength ${}_sH_R$. In Hull's presentation, K is intended to represent the effect of reward upon the learning process. However, is it reasonable to suppose that the effect of a given reward is uninfluenced by the animal's experience of it? Is it not possible that the animal in question would "become accustomed" to the reward and that its effect would be, to that degree, reduced? In answer, we offer a translation of the Hullian analysis, a translation in which the mathematical relations of the revision are retained intact, despite the fact that we conceive of the growth process in different terms. Before we can perform this translation, however, a more careful review of the Hullian position will be necessary.

Suppose we run a hungry animal through a series of trials in each of which he responds with R to a stimulus situation S and receives w grams of food. Suppose, furthermore, that this is the first time the animal has found himself in a situation like S, or, which is the same thing, suppose that ${}_sE_R$ is zero at the beginning of the series. In relation to the present discussion the revision contains three relevant postulates:

(1) Habit strength ${}_sH_R$ is a simple growth function of the number of trials N, or ${}_sH_R = 1 - 10^{-aN}$ (Postulate 4).

(2) Incentive function K is a negatively accelerated increasing function of w, or $K = 1 - 10^{-b\sqrt{w}}$ (Postulate 7).

(3) Excitatory potential ${}_sE_R$ is an increasing linear function of the product of ${}_sH_R$ and K, or ${}_sE_R = d \cdot K \cdot {}_sH_R$ (Postulate 9).

In expression (3) the constant d includes the variables: drive, stimulus dynamism, and delay of reinforcement. We lump them all together as d since they are all supposed to be constant throughout the series.

Our translation introduces two new terms: *effective reward* A and *expectation* P. Habit strength and incentive function are dropped. Excitatory potential, on the other hand, is retained. To begin at the beginning: effective reward in the first trial $A^{(1)}$ is determined solely by w, and, as a result of the first trial, expectation acquires an initial increment $\Delta P^{(1)}$ which is nu-

[1] The writer wishes to express his gratitude to Professor John P. Seward for his criticism and encouragement in the writing of this paper.

merically equal to $c \cdot A^{(1)}$, where c is an arbitrary constant, $0 < c < 1$. However, in accordance with our basic idea, effective reward in the second trial $A^{(2)}$ will be partially determined by $\Delta P^{(1)}$. More precisely, $A^{(2)}$ will be numerically equal to $A^{(1)}$ minus $\Delta P^{(1)}$. In the second trial, expectation acquires a second increment $\Delta P^{(2)}$ numerically equal to $c \cdot A^{(2)}$. This new increment turns right around and partially determines $A^{(3)}$ which, in turn, serves as the basis for still a third increment in expectation, and so on. In brief, expectation acquires an increment in each trial by virtue of the effective reward *for that trial.* Having done so, this increment determines, in part, effective reward in the next trial. In this sense, expectation is a mechanism of adjustment, compensating in each trial for a constant fraction of the existing effective reward. This process of mutual interaction and adjustment is summarized in our first postulate. In the Nth trial,

Postulate 1:

$$\text{A.} \quad \Delta P^{(N)} = c \cdot A^{(N)},$$

and

$$\text{B.} \quad A^{(N)} = A^{(N-1)} - \Delta P^{(N-1)},$$

where c is as above. We have said that effective reward in the first trial $A^{(1)}$ was a function of w. In our second postulate we specify that function.

Postulate 2:

$$A^{(1)} = c \cdot [1 - 10^{-b\sqrt{w}}],$$

where b, we shall suppose, is the same constant that appears in Postulate 7 of the revision, and c is the familiar constant of our previous analysis.

So far we have not mentioned the relationship between ${}_sE_R$ and either of our two variables. As might be expected, it is effective reward which is crucial in this connection. In any given trial we suppose that excitatory potential acquires an increment $\Delta {}_sE_R$ directly proportional to the effective reward for that trial. In other words, in the Nth trial, excitatory potential acquires an increment.

Postulate 3:

$$\Delta {}_sE_R = d \cdot A^{(N)},$$

where d is the same as in expression (3) above. It remains to justify our assertion that the mathematical relationships of the revision remain intact under our translation. As a consequence of Postulate 1, we may write

$$A^{(N)} = A^{(1)} \cdot (1 - c)^N. \tag{4}$$

Applying Postulates 2 and 3, we have

$$\Delta {}_sE_R = d \cdot [1 - 10^{-b\sqrt{w}}] \cdot [c(1 - c)^N], \tag{5}$$

and, performing the summation, excitatory potential after N trials

$${}_sE_R = d \cdot [1 - 10^{-b\sqrt{w}}] \cdot [1 - (1 - c)^N]. \tag{6}$$

If we now compare expressions (6) and (3), we see immediately that the first bracketed expression in (6) is the same as K in (3). Since the second bracketed expression and ${}_sH_R$ are both growth functions of N, we need only set

$$a = \log_{10} \frac{1}{1 - c} \tag{7}$$

to establish their numerical equality. In short, our translation has not affected the numerical value of ${}_sE_R$.

To summarize: Finding the static ideas of habit strength ${}_sH_R$ and incentive substance K questionable, we have offered a translation of the growth process in which ${}_sH_R$ and K are replaced by the dynamically interrelated conceptions of expectation P and effective reward A.

II. CHANGE OF REWARD

Before we go further, we must point out that throughout this paper we shall treat punishment and reward as if they fell along the same dimension, i.e., we shall arbitrarily suppose that effective reward is, in the case of punishment, negative and, in the case of reward, positive. One might well ask: what does "negative effective reward" or, for that matter, "positive effective reward" mean? Essentially, we mean that negative effective reward tends to weaken the appropriate stimulus-response connections and that positive effective reward tends to strengthen them. The exact nature of this "weakening" and "strengthening" will be taken up later.

To talk of a change of reward implies that we have two series alike in every respect except that in the first the animal receives a reward u_1 and in the second a different reward u_2. In the Hullian analysis, incentive function in the second series K_2 is dependent upon u_2 alone, i.e., the effect of u_2 is completely independent of the animal's previous experience of u_1. The effect of a present reward is entirely unaffected by the experience of past rewards. According to the present view, the animal enters the second series with an expectation P_1, acquired through experience of u_1 in the first series. Furthermore, this expectation will partly determine the effective reward in the first trial of the second series $A_2^{(1)}$. In other words, the experience of past rewards enters importantly into the effect of a present reward. To understand more clearly how P_1 determines $A_2^{(1)}$ we rewrite P_1 as $m \cdot A_1^{(1)}$ where $A_1^{(1)}$ is the effective reward in the first trial of the first series and

$$(1) \qquad m = 1 - (1 - c)^N,$$

N representing the number of trials in the series. In this reformulation, m may be thought of as the *magnitude* or strength of the expectation P_1. Similarly, we may think of u_1 as the *content* or object of P_1. Moreover, P_1 is completely determined by m and u_1. Hence, to say that $A_2^{(1)}$ is partly determined by P_1 is to say that $A_2^{(1)}$ is a function of m and u_1 as well as of u_2.

In greater detail, then, how do we suppose u_1, u_2, and m to determine $A_2^{(1)}$? Our first supposition is that if $m \cdot u_1$ is greater than u_2, $A_2^{(1)}$ will be negative. In other words, non-reinforcement or less reinforcement following reinforcement functions exactly like punishment. Similarly, if u_2 is greater than $m \cdot u_1$, we suppose that $A_2^{(1)}$ is positive and reinforcement conditions obtain. Thus, the *direction* of the change of reward is all-important in determining whether the effect of u_2 will be to punish or reward. In determining the extent of this effect, the influence of u_1 will be tempered by m. We suppose that the absolute value of $A_2^{(1)}$ is a simple growth function of the absolute difference between $m \cdot u_1$ and u_2. These suppositions find mathematical expression in our fourth postulate.

Postulate 4:

$$A_2^{(1)} = c \cdot [1 - 10^{-a(u_2 - m \cdot u_1)}], \quad \text{if } u_2 > m \cdot u_1,$$

or

$$A_2^{(1)} = -c \cdot [1 - 10^{-a(m \cdot u_1 - u_2)}], \quad \text{if } m \cdot u_1 > u_2,$$

where c is the same as in the first section.

The revision concerns itself with two basic types of experimental procedures: (a) reinforcement conditions in which the animal is run to a reward in a novel situation, and (b) non-reinforcement conditions in which the animal experiences a second series of trials where the previous reward is en-

tirely absent. Under reinforcement conditions, as we have already seen in the first section, the present postulates are strictly equivalent to Hull's mathematically. Under non-reinforcement conditions, effective reward in the second series will be negative and, therefore, so will "excitatory" potential. In this event, we rename negative excitatory potential $-{}_sE_R$ "natural inhibition" ${}_sI_R$ and set ${}_sI_R$ equal to the absolute value of $-{}_sE_R$. The use of the notation ${}_sI_R$, the same as for conditioned inhibition, is intentional. Unfortunately, we cannot show strict mathematical equivalence between natural and conditioned inhibition as we did in the case of the two interpretations of excitatory potential. The fundamental reason is that nowhere, neither in the *Principles of Behavior* (4) nor in their revision, does Hull specify a precise mathematical function for conditioned inhibition. Natural inhibition, on the other hand, is a simple growth function of the number of non-reinforcement trials. Nevertheless, we may substitute natural for conditioned inhibition in all the Hullian formulae without contradiction. More precisely, we may interpret the notation ${}_sI_R$ in the equations,

$$\dot{I}_R = {}_sI_R + I_R, \tag{2}$$

and

$$\dot{I}_R = 1 - 10^{-\omega \dot{n}}, \tag{3}$$

as natural inhibition without contradicting ourselves. In the present approach, the notions of reactive inhibition I_R and total inhibition $\dot{I}_R$ are retained. Only conditioned inhibition is abandoned. The substitution of natural for conditioned inhibition involves one minor discrepancy. Hull leaves open the possibility that conditioned inhibition might be generated under reinforcement conditions. On the other hand, natural inhibition is definitely not generated under reinforcement conditions. However, to date there are no experimental findings which necessitate the assumption of conditioned inhibition as well as reactive inhibition during reinforcement. Surely the analysis of massed vs. distributed practice and reminiscence offered in the *Principles of Behavior* necessitates no such assumption.

Before we can close, we must guarantee that excitatory potentials generated by change of reward will not summate to a value greater than the physiological maximum M. This we can do by simply supposing that two excitatory potentials ${}_sE_{1R}$ and ${}_sE_{2R}$ generated on the two series involved in a change of reward summate algebraically unless the algebraic sum is greater than M. In this case, the two potentials summate to equal M. Clearly, this process is not very plausible and in a more finished work it would surely be modified. A more reasonable summation procedure could be introduced without affecting the validity of the arguments which follow, though the derivations would be considerably more complicated. In the interests of simplicity, then, we use the summation procedure described above, not forgetting that it is only a crude, first approximation.

III. CONTRAST EFFECT

The first experimental test to which we subject our postulates is Crespi's "contrast effect" (1). Crespi's experiment may be divided into two series of trials. In the first, he ran five groups of albino rats under constant drive conditions down a straight runway to a goal box where the rats received immediately 1, 4, 16, 64, or 264 units of food according as they were in group 1, 2, 3, 4, or 5. Taking mean time in the runway as his criterion of response latency, Crespi continued the first series of trials until

the response latency of all five groups had become asymptotic. In the second series of trials, all five groups received 16 units of food and the series was continued until response latency had once more become asymptotic. We denote the asymptotes of this second series by L_1, L_2, L_3, L_4, and L_5. The results of the experiment are:

$$L_3 > L_1, L_3 > L_2, \tag{1}$$

and

$$L_3 < L_4, L_3 < L_5. \tag{2}$$

As Crespi points out, these results do not follow from Hull's postulates.

Towards an explanation in terms of the present theory, we note first that under the conditions of the experiment, i.e., D[2], V, and V_1 held constant and $J = 1$, excitatory potential is asymptotic to

$$b \cdot [1 - 10^{-au}], \tag{3}$$

where $b = D \cdot V \cdot V_1$ and u is the amount of reward. Since the animals are running to food, we follow Hull and set $u = \sqrt{w}$. Hence, in the first series of trials for Group 2 the asymptote is

$$b \cdot [1 - 10^{-a\sqrt{4}}] \tag{4}$$

and for Group 3

$$b \cdot [1 - 10^{-a\sqrt{16}}]. \tag{5}$$

In the second series of trials for group 2 a change of reward takes place and a second excitatory potential is generated where the asymptote equals

$$b \cdot [1 - 10^{-a(\sqrt{16} - \sqrt{4})}] \tag{6}$$

by Postulate 4. The asymptote for the rats of Group 3 remains as in (5) since there has been, for them, no change of reward. Hence, at the end of the second series excitatory potential for Group 2

$$E_2 = b \cdot [1 - 10^{-a\sqrt{4}}] + b \cdot [1 - 10^{-a(\sqrt{16} - \sqrt{4})}] \tag{7}$$

and excitatory potential for group 3

$$E_3 = b \cdot [1 - 10^{-a\sqrt{16}}]. \tag{8}$$

It is a property of negatively accelerated increasing functions that

$$[1 - 10^{-a(u_2 - u_1)}] > [1 - 10^{-au_2}] - [1 - 10^{-au_1}], \tag{9}$$

unless u_1 equals zero. Taking $u_2 = \sqrt{16}$ and $u_1 = \sqrt{4}$ and substituting in (7) and (8), we have

$$E_2 > E_3. \tag{10}$$

Similarly, we may establish

$$E_1 > E_3. \tag{11}$$

From Postulate 15 of the revision we may now assert

$$L_3 > L_1, L_3 > L_2. \tag{12}$$

Throughout this derivation we have (a) assumed that, for example, excitatory potential for Group 2 at the end of the first series equals the asymptotic value when, as a matter of fact, it only approximates it, and (b) neglected reactive inhibition. However, neither of these considerations affects the conclusion.

So far we have established Crespi's "elation effect." The derivation of the complementary "depression effect," i.e., (2) above, proceeds as with elation, until we show that at the end of the second series excitatory potential for Group 4

$$E_4 = b \cdot [1 - 10^{-a\sqrt{64}}] - b \cdot [1 - 10^{-a(\sqrt{64} - \sqrt{16})}] \tag{13}$$

and for Group 5

$$E_5 = b \cdot [1 - 10^{-a\sqrt{256}}] - b \cdot [1 - 10^{-a(\sqrt{256} - \sqrt{16})}]. \tag{14}$$

Once more applying (9) and Postulate

[2] D represents the drive state, V the stimulus dynamism or "intensity" of the conditioned stimulus, V_1 the stimulus intensity during the learning process, and J reflects delay in reinforcement.

15, we have

$$(15) \qquad L_5 > L_3, L_4 > L_3.$$

It is clear from the derivation that our demonstration of Crespi's results rests essentially upon Postulate 4 and expression (9). Our derivation is, therefore, a "pure" theorem, i.e., it does not rest upon a supposed difference in constants.

IV. PUNISHMENT

In his *Experimental Study of Punishment* (2) Estes ran a variety of experiments, five of which, Experiments A, B, C, D, and I are of immediate relevance to the present paper. In all five experiments Estes divided his rats into a control and an experimental group. Both groups were reinforced in the Skinner box until the bar-pressing habit had become well-established, and to approximately the same degree, in the two groups. In Part 1 of the extinction procedure the experimental animals were punished in any one of five different manners depending upon the experiment, while the control animals experienced simple non-reinforcement trials. In Part 2 of the extinction procedure both groups were put on the non-reinforcement routine. In other words, the control animals were extinguished throughout in the usual fashion while the experimental animals received punishment in some form or other in the earlier part of extinction and, in the later part, joined the control animals on the non-reinforcement procedure. Estes concerned himself with both the total number of responses in extinction and the rate of responding as measured by the number of responses in an experimental session. We shall be concerned only with the rate of responding.

In Experiment A the experimental animals received a single short period of mild punishment in Part 1 of the extinction procedure, in Experiment B they experienced a single short but severe punishment, and in Experiment D they were subjected to several short periods of severe punishment. In all three experiments the results were essentially the same: namely, (a) in Part 1 the rate of responding fell far more rapidly among the experimental than among the control animals, and (b) in Part 2 the rate of responding for the experimental group rose more or less abruptly until it had rejoined, and in some instances excelled, the rate of responding in the control group. Estes accounts for these results by supposing that punishment produces only a temporary weakening in habit strength, most probably through the mediation of emotional disturbances, from which the habit will completely recover when punishment is removed. The present approach offers an alternative explanation.

In the learning procedure both groups generate excitatory potential from the sequence: presentation of the conditioned stimulus S (being in the Skinner box and hungry), performance of the critical response R (pressing the bar), and receipt of reinforcement u_R (presence of the food pellets). Throughout the extinction procedure the control animals are placed on the sequence: S, R, and O, i.e., no reward no punishment. Hence, among the control animals natural inhibition will be generated by virtue of the change of reward from u_R to O, and the response will extinguish concomitantly. The experimental animals, on the other hand, are subjected to two distinct sequences in Part 1 of the extinction procedure. The first is the same as for the control animals: namely, S, R, O, and we expect the same effects. The second sequence

is: S, R, and u_P (shock). On the basis of this second sequence, inhibition is built up which, in combination with the non-reinforcement inhibition of the first sequence, will produce a faster rate of extinction among the experimental than among the control animals. In Part 2 of the extinction procedure the first sequence continues to generate inhibitory potential equally in the two groups. However, the second sequence, S, R, u_P, becomes S, R, O, and since a change of reward has taken place from u_P to O, an excitatory potential will build up among the experimental animals. This latter potential will produce the more or less abrupt improvement in response rate for the experimental group which was to be explained. In brief, though the experimental animals are punished in Part 1, they are rewarded in Part 2 by non-punishment and, therefore, their response rate will rise until it rejoins the simple non-reinforcement curve of the control animals.

Experiment C of Estes' study presents results which the present approach does not explain. In this experiment the experimental animals were subjected to a prolonged period of punishment in Part 1 followed by an 8-day lapse before Part 2 was begun; in Experiments A, B, and D the inter-part lapse was one day. Under these conditions, the recovery in response rate predicted by Postulate 4 did not occur. The source of the difficulty lies in our failure to consider the temporal separation of our first and second series in the analysis of change of reward. Inasmuch as we have not approached the problem at all, ours is an error of omission rather than commission. This being the case, we will leave the question of simple temporal decay to a more complete presentation of the theory.

In Experiment I the experimental animals received punishment at 30-second intervals in Part 1, care being taken that shock did not occur during or immediately after the bar-pressing response. The results were essentially the same as in Experiments A, B, and D. However, though shock did not immediately follow bar-pressing, it did *follow* it, and rather quickly at that. In the present approach the principle of secondary "reward" is elaborated to include punishment, i.e., a stimulus constantly associated with punishment acquires punishing properties. Applying this principle to the problem at hand, we see that bar-pressing is punished, though to a slightly lesser degree than in the previous experiments. True, almost any response is punished by indiscriminate shock but whether or not other responses were punished is not pertinent to the analysis. When, in Part 2, the experimental animals were not punished, excitatory potential built up as in Experiments A, B, and D and the empirically observed rise in response rate follows as a theorem of the theory.

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SOME CRITICISMS OF "A MATHEMATICAL MODEL FOR SIMPLE LEARNING"

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Recently R. R. Bush and F. Mosteller[1] reported an important contribution towards the mathematical organization of behavior theory. Because this work is so important, it deserves the most severe logical criticism. The present writer has found one logical flaw in their paper which can be remedied by changing a few words. Although this error in reasoning apparently has no effect upon the logic of the further deductions in their paper, it is possible that future work might be invalidated because of this fallacy. The purpose of this paper, therefore, is to correct this one highly technical mistake.

The theory of Bush and Mosteller is based upon the probability (p) that a response will occur in a specified time (h). The probability that the response will occur in the next period of time is then obtained by applying an operator Q to p. From certain assumptions about Q they obtain the approximation

$$Qp = p + a(1 - p) - bp. \quad (1)$$

In this equation p and Qp are probabilities, while a and b are parameters. Then they make the statement which we propose to criticize:

> To maintain the probability between 0 and 1, the parameters a and b must also lie between 0 and 1 (p. 315).

The context makes it clear that the term "probability" in the quotation refers to Qp.

[1] R. R. Bush, & F. Mosteller, A mathematical model for simple learning. PSYCHOL. REV., 1951, 58, 313–323.

Since p is a probability, by convention, $0 \leqq p \leqq 1$. Their statement may then be rephrased as follows: If $0 < Qp < 1$, then *both* $0 < a < 1$ and $0 < b < 1$. This implies that if it is false that *both* $0 < a < 1$ *and* $0 < b < 1$, then it is false that $0 < Qp < 1$.

But it is easy to show by simple arithmetic that this is not true. For example let us choose the following values: $p = 0.5$, $a = -8.4$, $b = -9.0$. Then from their basic equation we calculate $Qp = 0.8$. Since our value of p is within its proper range while our values of a and b are clearly outside their proper range, it should be impossible, according to Bush and Mosteller, for Qp to lie between 0 and 1. And yet it does. What is wrong?

The answer is simply this: Bush and Mosteller have confused *an incorrect necessary* condition with a *correct sufficient* condition for $0 < Qp < 1$. It is *not* true to say that if $0 < Qp < 1$, then both $0 < a < 1$ *and* $0 < b < 1$. It is true, however, to say that if we *postulate* that both $0 < a < 1$ and $0 < b < 1$, then it follows that $0 < Qp < 1$.

This later statement is not obvious and deserves proof. We postulate the following conditions:

$$0 \leqq p \leqq 1, \quad (2)$$

$$0 < a < 1, \quad (3)$$

$$0 < b < 1. \quad (4)$$

First let us show that $0 < Qp$. By subtracting b from each term of Formula (4),

$$0 < 1 - b. \quad (5)$$

By multiplication of Formulas (5) and (2),

$$0 \leqq (1 - b)p, \tag{6}$$

where the equal sign holds if and only if $p = 0$.

By subtracting p from each term of Formula (2),

$$0 \leqq 1 - p, \tag{7}$$

where the equal sign holds if and only if $p = 1$.

By multiplication of Formulas (3) and (7),

$$0 \leqq a(1 - p), \tag{8}$$

where the equal sign holds if and only if $p = 1$.

By addition of Formulas (6) and (8),

$$0 < (1 - b)p + a(1 - p). \tag{9}$$

The equal sign is absent from Formula (9) because if that sign applies to either of Formulas (6) and (8) it does not apply to the other.

Since the right-hand member of Formula (9) equals Qp, therefore for all values of p in the closed interval (0, 1),

$$0 < Qp. \tag{10}$$

Now we can deduce that $Qp < 1$.

By multiplication of Formulas (4) and (2)

$$0 \leqq bp, \tag{11}$$

where the equal sign holds if and only if $p = 0$.

If we exclude the case where $p = 1$, Formula (7) becomes

$$0 < 1 - p. \tag{12}$$

Dividing Formula (11) by $1 - p$ in Formula (12),

$$0 \leqq \frac{bp}{1 - p}. \tag{13}$$

$$\therefore \quad 1 \leqq 1 + \frac{bp}{1 - p}. \tag{14}$$

From Formulas (3) and (14),

$$a < 1 + \frac{bp}{1 - p}. \tag{15}$$

We have excluded the case where $p = 1$ because the ratio added to 1 in the right-hand side of Formula (15) is not defined for this value of p. Therefore,

$$a < \frac{1 - p + bp}{1 - p}. \tag{16}$$

Formula (12) allows us to multiply both sides of Formula (16) by $1 - p$ without changing the direction of the inequality. Therefore

$$\begin{aligned} a - ap &< 1 - p + bp, \\ p + a - ap - bp &< 1. \end{aligned} \tag{17}$$

From Formulas (1) and (17),

$$Qp < 1, \quad (p \neq 1). \tag{18}$$

Now we can consider the case when $p = 1$. Substituting this value of p into Formula (1),

$$Qp = 1 - b, \quad (p = 1). \tag{19}$$

By adding $1 - b$ to each term of Formula (4),

$$1 - b < 1. \tag{20}$$

From Formulas (19) and (20),

$$Qp < 1, \quad (p = 1). \tag{21}$$

Formulas (18) and (21) together imply that for all values of p in Formula (2),

$$Qp < 1. \tag{22}$$

Combining Formulas (10) and (22),

$$0 < Qp < 1.$$

Q.E.D.

We suggest therefore that the disputed statement be modified as follows:

To maintain the probability (Qp) between 0 and 1 it is *sufficient* to

postulate that both a and b lie between 0 and 1.

The writer wishes to make one last comment. He believes that if psychology is to become a science with high predictive power it must be organized by means of logic and mathematics. The work of Bush and Mosteller and of many others contributes towards this end. Until the average level of mathematical competence of psychologists is much greater than it is now, however, it is unreasonable to expect them to follow the deductions of equations when the proofs are not set forth step by step. The writer believes that if an equation is important enough to be stated, it should either be postulated or else clearly deduced.

It is earnestly hoped, therefore, that in future reports of their work Bush and Mosteller will explain their derivations in greater detail. If they believe that a long deduction will confuse the reader, they can always put it in a mathematical appendix at the end of their paper. The writer hopes, in closing, that Bush and Mosteller and the other readers will receive this paper in the spirit of friendly cooperation and criticism in which it was written.

[MS. received October 31, 1951]

APPROACH AND AVOIDANCE; A REPLY

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Dr Bitterman's new data (1) are readily accounted for by the approach-avoidance formulation. They present rather less difficulty than does the successive discrimination problem which, as I initially pointed out (2), is described more "simply" in terms of movement-learning. As long as all such performances can be handled by a single set of descriptive terms, the fact that some (but not all) of them can be handled also by a different set is no argument against unitary description. Dr. Bitterman does not attempt to refute my analysis of how the observed behavior can be described as approach-avoidance, but instead tries to show the inadequacy of doing so. His argument leans heavily on the unsubstantiated assumption that response to a stimulus-compound is necessarily more difficult than to a configuration encompassing two stimulus-objects ("certain global properties of a stimulus situation"). The data do not tell us whether the animals responded configurationally, relationally, or absolutely, and this is, in any case, irrelevant to the present issue (3, section E).

The new data demonstrate that, with no necessity for responding to the relatively subtle wide-narrow and dark-light differences, the *B* animals responded to the more obtrusive positional (left-right) and striped-nonstriped cues.[1] In approach-avoidance terms, the *B* animals learned approach to the cards $S_{striped}S_{left}$ and $S_{nonstriped}S_{right}$, and/or avoidance of cards $S_{striped}S_{right}$ and $S_{nonstriped}S_{left}$. There was, as we should expect, immediate transfer from WN to NW and from LD to DL. Problem *A* should be, and was, more difficult than *B*, first because it required differentiation of narrow from wide, dark from light (not required in problem *B*), and second because the prepotent stimulus-aspect, position, was ambivalent and had to be "disregarded" in *A*, whereas in *B* it was the effective intra-trial ("within-trial") cue. Since the *A* animals were required to abstract the relevant cue from the total stimulating situation, they had to deal with four, instead of only two, different stimulus-presentations.

The answer to Dr. Bitterman's first question (1, 5th paragraph) is that his *B* animals did not "consistently approach N." They did not make the unnecessary W–N differentiation, but learned approach to striped-left, rather than to wide-left as he suggests. Since W and N were both striped (in contrast to L and D), WN and NW were functionally equivalent, as shown by the performance of the rats. The second question makes the implicit assumption that if response to a stimulus-compound is or can be learned, responses to the elements of that compound are, *ipso facto*, not learned. This problem was discussed at length in my previous paper (3, section D). My transfer data show that the animals did learn approach to white, white being the sufficient and only effective cue and the only component of the stimulating situation which was consistently rewarded. This does not mean that the same animals could not as readily learn a successive problem in which response to a combination of directional and nonspatial cues is

[1] The former were probably available as external—my type (a)—as well as kinaesthetic—my type (b)—cues, although this cannot be determined with certainty from the condensed description given.

necessary for 100 per cent success. The third question may be answered by pointing out that generation of different kinaesthetic stimuli (which may serve the function of cues) as the animal moves this way or that, is not inconsistent with learning approach to a stimulus-object which from time to time occupies different locations in space. It is the *learned response* which in this sense is considered identical, not the appropriately varying movements which instrumentally subserve that response.

In the rest of his paper Dr. Bitterman largely reiterates the position taken by Weise and Bitterman (4),with little reference to my subsequent exposition (3). My discussion of the irrelevance of an experiment "designed to study the relative difficulty of simultaneous and successive discrimination learning" for the quite separate question of whether the learned response is a non-directed muscle movement or a goal-directed act of approach, is not considered. In his first paragraph Dr. Bitterman attributes to me the "assumption that the process of response-selection need not be considered in the analysis of discrimination learning," but in the next paragraph says that "Nissen's approach . . . defines discrimination as a process of response-selection." Although neither is unobjectionable, I rather prefer the second thought, since the whole point of my examination has been a consideration of alternative possibilities in an attempt to identify the nature of the learned differential response. It is difficult to see how Dr. Bitterman reconciles my alleged "refusal to consider the qualitative variations in response" with the fact that my experiment as well as my criticisms of the movement-learning formulation (2, pp. 122, 129) are based on the observation that movements are precisely adjusted to the requirements of an approach or avoidance. He maintains that I ignore (whereas he presumably does not ignore) the problem of perceptual development. As I see it, none of the data considered in this series of papers makes any contribution to the problem of perceptual *development*. The only experimental finding presented in this connection is that one problem is easier to learn than another (by rats 85 to 100 days old); this hardly justifies the conclusion that the former is more "primitive."

In locating the development of sensory-motor coordinations subserving approach-avoidance at an ontogenetic stage preceding that in which the animal is or can be tested in the conventional two-choice discrimination situation, I have not oversimplified matters as Dr. Bitterman suggests. As a matter of fact, he objects strenuously to the complexities entailed in the approach-avoidance formulation. So the issue is not simply an "of course" one of "simplicity versus complexity," although it is related to the question of what the simplicities and complexities are, and where they lie. The basic issue remains as I have stated it (2, 3): whether the learned discriminative response in the conventional two-choice (or multiple choice) problem may be appropriately described as approach-avoidance, or whether a different set, or multiple sets, of descriptive categories must be used.

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GILINSKY'S THEORY OF VISUAL SIZE AND DISTANCE

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In a recent issue of this JOURNAL Gilinsky (3) presented a theory of visual size and distance. The basic elements of the theory consisted of the generation and validation of two formulas—one for perceived size and the other for perceived distance. She asserted that "the general validity of the two formulas is supported by visual size and distance experiments planned and conducted for this study" (p. 460). Furthermore, it was claimed that additional checks on the formulas were made by subjecting size-distance data of other investigators to the functions in order to see how well fact agreed with theory. It was concluded that "this variety of evidence indicates that the derived relations and the unifying law which they express may have considerable generality" (p. 460). While the writer does not wish to reject this tempered conclusion categorically, he would like to present some reasons why the "derived relations and the unifying law" may *not* have considerable generality. These reasons will take the form of appraisal of certain concepts and assumptions underlying the formulation, and the inspection of certain aspects of methodology. The adequacy of some of the empirical data used to validate the theory will be questioned and assessed.

The two basic formulas are shown to be derivable from three different sources, all yielding the same result. One of the three, with which most of this note will concern itself, deals with size-distance relations and size constancy. Obviously, what is said about this particular source applies equally to the others insofar as critical assumptions about perceived size and distance are concerned.

The notable feature of the derivations on the basis of size constancy principles is the limitation imposed on the one hand by the "law of size constancy" wherein perceived size remains invariant with changes in physical distance, and on the other, the limitation imposed by the "law of the visual angle," wherein perceived size is proportional to physical distance. The restriction imposed by these two laws therefore forces the expression of any size-distance relation to fall somewhere between these two extremes. Gilinsky says (p. 469): ". . . beginning with the first systematic experiment on perceived size . . . , the size matches *consistently* follow an *intermediate law*" (first italics mine). This statement is not strictly true. Data which show "over-constancy" are not included in the formulation. To ignore such data may have been easy at the time Holway and Boring (4) ascribed "over-constancy" to a space error. Since that time, however, similar data have been found by Gibson (2), by Chalmers (1), and recently by the writer (5). What looked like an artifact may be a genuine perceptual fact, at least for binocular vision. Each of these four studies obtained the data on "over-constancy" for binocular vision. The experiments of Gibson (2) and Smith (5) were performed in outdoor environments under normal daylight illumination. The other two studies were performed indoors with some restriction on the visual field. Binocular vision may be a special case. However, Gilinsky's formulation is a general expression and should

apply. It is curious that she should ignore or try to explain away "overconstancy," for the problem is, after all, only one of localization. Objects may be localized in visual space too near or too far with respect to physical distance. If the latter obtains, apparent size will be too large, or in relative terms, larger than it would be if the object were localized correctly. This principle has been known at least since Helmholtz's time, yet somehow it has not found its way into the general understanding of size-distance relationships. Gilinsky's formulation implies that O always falls short of the mark or hits it (at short distances), but never overshoots it, i.e., objects are not seen farther away than their actual physical distance—which is plainly not true.

The lack of isometry between perceived and physical distance (except at short distances) is a crucial part of the formulation. Gilinsky states, "Perceived distances are foreshortened" (3, p. 462). Now this statement may be true as far as the "law" of visual perspective is concerned, and it may be in agreement with various mathematical models of visual space. But it does *not follow* from evidence regarding size-constancy. Such evidence has never shown that constancy holds for great distances, nor has it shown that it holds *only* for very short distances. Yet it is stated (3, p. 463) that, ". . . the transformation from physical to visual space is cubic or isometric at short distances such as the *normal viewing distance*" (italics mine). Taken literally, this means that only at this distance does perceived size equal true size. Beyond this point perceived size must diminish with distance at some specified rate. Furthermore, it is not clear as to what physical distance constitutes "normal viewing distance," although one would gather that it is not more than a few feet. This concept of "normal viewing distance" is a difficult one. By what operations does one specify this distance? The problem here concerns real space and real objects of perception, not an ideal space defined by formulas in a mathematical model. Is the normal viewing distance of an orange 2 feet? Perhaps. A person? A house? A mountain? It would seem to be reasonable to assume that this distance would vary with the object. If so, what happens if it varies beyond the short distance of isometry assumed by the formulation? According to the formulation, $s = S$ (perceived size equals subjective true size) when viewing distance is not zero but some small value, δ. But if normal viewing distance is not some small value, Gilinsky's expression for perceived size can not be generated, since at some point an increasing value of δ becomes incompatible with the assumption of non-isometry beyond short distances. In addition to this basic difficulty, there still remains the problem of accounting for size-constancy data which have demonstrated the equality of perceived size and true size with binocular vision for distances considerably greater than a few feet. The distance at which isometry no longer holds is a controversial issue, but most investigators would certainly agree that it is at a distance greater than a few feet. The main point is that the kind of visual space that Gilinsky describes can not be based on size-constancy data. Curiously, however, Gilinsky uses such data as a check on her theory. This point will be taken up later.

The question of isometry and normal viewing distance can not be considered apart from the meaning and significance of Gilinsky's parameter A. This parameter, common to both basic equations is defined as "the maximum limit of perceived distance for a given O under given conditions" or "A is the ap-

parent distance of objects at infinity" (3, p. 466). Mathematically

$$A = d,$$

when

$$D = \infty,$$

where D = physical distance and d = perceived distance.

By definition it is *only* when

$$A = \infty$$

that

$$d = D.$$

However, note again that (p. 463), "the transformation from physical to visual space is cubic or isometric at short distances such as the normal viewing distance 8." Therefore

$$d = D$$

and thus

$$s = S$$

where s = perceived size, S = subjective true size. The question is obvious. How can these two relations between physical and perceived distance be true at one and the same time? Consider the example in which the determination of A is a function of s, when s is taken as the apparent size of an astronomical object. The measure is derived from a variable which is observed presumably close at hand. That is to say, the visual sizes of the variable close at hand and of the astronomical object are taken to be equal, and the physical size of the variable then equals the apparent size of the object. We are told that "We assume equality of *visual* size when object and measuring rod have equal physical size and are held at identical viewing distance" (3, p. 463). How can the operation involved for obtaining A be compatible with this requirement? Furthermore, it is claimed that only the ratios of subjective magnitudes (s and S) are involved in the expression for size and distance, but this is clearly not true in the derivation of A. Just ask what A means. As an example, assume that A is said to equal 130 feet on the basis of observations of the apparent size of the sun. Does this literally mean feet? If not, what does it mean? If it does mean real feet, can it be said with certainty that the sun looks only 130 feet away, or, for that matter, 1,300 feet? In order to amplify this question, consider the methods of deriving A. Gilinsky lists seven methods. Two of the seven will serve as examples. The first method (3, p. 480) involves obtaining A from a size observation of an astronomical object where D is assumed infinite relative to A, although *the metric of the latter is unspecified*. The implication from the basic distance equation

$$\frac{d}{D} = \frac{A}{A + D}$$

is that the metric is the same as that for physical distance. With S_0 and D known, and s observed, φ (phi) is determined. Setting (from the basic distance formula)

$$d = A$$

the equation for A becomes

$$A = d = \frac{s}{\varphi}.$$

But note the significance of s in this equation; it stands for apparent or perceived size. In solving for A

$$s = S_0,$$

but by definition it is *only true* that this follows when

$$A \doteq \infty.$$

The same difficulty is met when deriving A from a size observation of a nonastronomical object. In this case, the value of A is determined by the basic size formula (3, Equation 3, p. 480). Again a contradiction is involved in the size measure. The size reduction ratio

used (s/S_0) substitutes S_0 for S although by definition $S_0 = S$ *only* when $D = \delta$. Therefore the derivation of A again becomes incompatible with the requirements of the size equation, except for the case when $D = \delta$.

These inconsistencies involving apparent and physical size and distance are not only important but perhaps indicative of the minimum amount of experimental verification offered by Gilinsky for her size function. Considerable evidence is given for the distance expression. Unfortunately, the size evidence presented, in the writer's opinion, is not very good evidence. It was pointed out previously why it seemed curious that Gilinsky should use size-constancy data to verify her expression for size and distance. The data she used came from the Holway and Boring study (**4**). It is necessary to point out that this study obtained size observations from 5 *O*s, and that the data were corrected because of a presumed space error. The corrected functions for binocular vision were very close to theoretical size-constancy and were considered linear by the authors, except perhaps the data for one *O* (EGB). This *O*'s data were not only curvilinear, but even when unadjusted, did not give the over-constancy typical of the other 4 *O*s. It is the data of this one *O* which Gilinsky selects as a "check" against her size expression. It can be shown, however, that the data of other *O*s in this study do not fare so well. As depth perception diminishes (binocular vision to monocular vision and artificial pupil in the Holway-Boring study) the value of parameter A likewise diminishes. With small values of A, *e.g.*, 25′, the theoretical size-distance function accelerates rapidly for a short time and then levels off (**3**, p. 468). The analysis of any of the reduced-cue data in the Holway-Boring study, which have small values of A, will convince the reader that the empirical data fit Gilinsky's expression of size very poorly, if at all. They simply can not, because within the distance

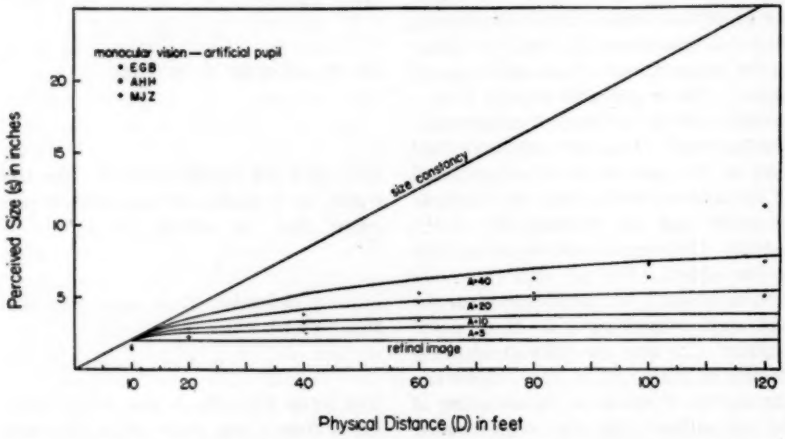

FIG. 1. Perceived size (s) as a function of physical distance (D) for monocular vision with artificial pupil. Data are from Holway and Boring (**4**). The graph is the same as that presented by Gilinsky (**3**, p. 470) except different data from Holway and Boring are plotted, and the theoretical functions involve different values of Parameter A. The observed data have been corrected according to the ratios given by Holway and Boring (**4**, p. 36). Perceived sizes for the theoretical functions were obtained by assuming various values of A and solving for perceived size (s) in Gilinsky's basic size expression.

studied the functions are linear. For purposes of comparison, Boring's binocular and monocular data were analyzed. Using the A value of 243′ given by Gilinsky for binocular vision, s was computed for 10′, 20′, 40′, 60′, 80′, 100′, and 120′. The observed measures (corrected by a factor of .8532,* assuming a space error) of perceived size were then compared with the theoretical values. Next a plot was made of the observed corrected data and a straight line was fitted. Values of s were determined by interpolation and then compared to the observed values. The variance ratio between these 2 sets of deviations is 5.39, which is significant between 1–5% level. Gilinsky's fit is favored. Again the reader is reminded that Boring was the only O who gave non-linear data for the binocular condition. The same procedure as described above was applied to the monocular data with an A value of 132′ (given by Gilinsky) and a correction factor of .8270. The variance ratio for these two sets of data is 6.22 which is also significant between the 1–5% level, but the fit favors the linear function in this case.

In the figure perceived size has been plotted as a function of distance in the same way as has been done by Gilinsky (3, p. 470). The solid curved lines represent theoretical functions for four different assumed values of A; 5′, 10′, 20′, and 40′. The plotted points are observed corrected data (monocular vision-artificial pupil) for three Os in the Holway-Boring study (EGB, AHH, and MJZ). This figure shows clearly that a linear function would fit better than any curvilinear one, for Os individually or taken together. This evidence along with that presented above is ample reason for concluding that the Holway-Boring data do *not* agree with Gilinsky's theory, and therefore hardly serve as experimental confirmation.

* The correction factors were determined by using the values given by Holway and Boring (4, p. 36).

SUMMARY

An attempt has been made to evaluate certain aspects of Gilinsky's theory of perceived size and distance. The evaluation has concerned itself principally with size-constancy relationships.

(1) The restriction of theory to the cases where perceived size is proportional to physical distance requires that a number of data be ignored or explained away. The formulation specifically denies that perceived size may increase with distance, a denial which is contrary to fact.

(2) The use of the concept of "normal viewing distance" in the formulation raises some serious questions about the generality of the theory.

(3) Certain inconsistencies are apparent in the derivation of the theory on the basis of size-constancy principles.

(4) Both the derivation and meaning of the A parameter in the theory are questionable.

(5) Even allowing the restriction noted in (1) above, the theory is *not* confirmed by experimental evidence.

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GILINSKY'S EQUATIONS FOR PERCEIVED SIZE AND DISTANCE

BY GLENN A. FRY

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In a recent paper Gilinsky (**3**) presents equations for perceived size and perceived distance which she claims to have derived from Luneburg's basic metric of visual space (**4**), and also from principles of perspective.

However, her basic proposition that

$$\frac{ds}{d} = d\phi \quad \text{or} \quad \frac{s}{d} = \phi \qquad (1)$$

does not come from Luneburg. In order to derive this equation from Luneburg's

$$ds^2 = \overline{d(d)}^2 + M^2(d\phi^2 + \cos^2\phi\, d\theta^2) \qquad (2)$$

it is necessary to assume that

$$M = d \qquad (3)$$

and then for the special case in which $d(d) = 0$ and $d\theta = 0$, Equation (1) holds.

Assuming that $M = d$ is tantamount to assuming that visual space is Euclidian. Luneburg was not willing to restrict his theory by making such an assumption. Luneburg (**4**) defined M as follows:

$$M = \frac{1}{\sqrt{K}} \sin[(d)\sqrt{K}],$$

where K is a constant. It is only when K becomes very small or is equal to zero, that

$$M = d.$$

Furthermore, her second basic proposition that

$$d = \frac{a}{\gamma + \mu} \qquad (4)$$

is not derived from Luneburg's equation at all. Luneburg pointed out that d could be any function of γ but in his formulation of the relation between perceived and visual space he assumed that

$$d = \frac{2}{\sqrt{K}} \arctan[\sqrt{K} e^{-\sigma(\gamma+\mu)}],$$

where K, σ, and m are constants. Gilinsky derived from Equation (4) above the following equation

$$d(d) = -\frac{a\, d\gamma}{(\gamma + \mu)^2} \qquad (5)$$

and substituted it in Luneburg's equation (Equation [2] above); and then by limiting consideration to the case in which $d\phi = 0$ and $d\theta = 0$, Equation (2) reduces back again to Equation (5), and this in turn reduces back again to Equation (4) so that she ends up with what she starts out with.

Gilinsky's basic equation for perceived distance

$$d = \frac{AD}{A + D} \qquad (6)$$

can be derived directly from Equation (4), by assuming that $D = a/\gamma$ and that $A = a/\mu$.

It appears to the writer, therefore, that Gilinsky has belittled her own contribution by regarding her own equations as having been derived from Luneburg's. Actually she has found an equation which fits a reasonable amount of data and which is in agreement with the logically deduced law of complete size constancy for objects close at hand, and with the logically deduced law of "retinal image" for remote objects. Instead of deriving anything from Luneburg's theory she

has produced evidence that $ds/d = d\phi$, which indicates that visual space is Euclidian, and she has also provided a new distance function (Equation [6] above) which should probably be substituted for the one that Luneburg used.

The statement that Gilinsky has produced evidence that

$$\frac{ds}{d} = d\phi \tag{7}$$

should be explained further. She has determined by experiment that

$$d = \frac{AD}{A + D} \tag{8}$$

and that

$$s = \frac{\phi AD}{A + D}. \tag{9}$$

Combining these two equations,

$$\frac{s}{d} = \phi; \tag{10}$$

and taking the derivative,

$$\frac{ds}{d} = d\phi. \tag{11}$$

It follows from this that

$$M = d$$

and that visual space is Euclidian.

In deriving her basic equations from perspective principles Gilinsky has made the error of assuming that the dimensions A and d in Fig. A2 of her paper represent perceived distances. These are mere dimensions on a perspective drawing and only in a symbolic sense represent perceived distances. What one can deduce from perspective principles is that if perceived distance conforms to the relation expressed in Equation (6), then points lying on a straight line will be perceived as lying on a straight line regardless of the value of the constant A. However, the line may be oriented at a different angle in perceived space than in physical space, and changing the value of the constant A will change its apparent direction. In a separate paper the writer (2) has elaborated upon these relationships.

It should be noted that Equation (6) is the same equation which von Helmholtz (5, pp. 324–326) and von Kries (5, pp. 380–394) and the writer (1) have used in expressing the relationship between perceived size and perceived distance. These writers have deduced the relationship between perceived distance and physical distance from the basic principles of stereopsis.

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THE GIBSONIAN VISUAL FIELD

BY EDWIN G. BORING

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Gibson's comment (2) on my suggestion that the Gibsonian visual field may be nothing other than the Gibsonian visual world reduced by the elimination of the clues to distance (1) seems to me to leave the matter still not wholly clear. Let me try to explicate our views.

The reader must remember that Gibson thinks and writes as a phenomenologist, I as an operationist. Gibson thinks of a phenomenal world as there, sitting tight, ready to be described by the unprejudiced observer. I think of psychological facts as observed relations that are general and not particular, facts that have their exact meaning established by the generalized operations (not the particulars) by which they are observed. Gibson, I should guess, thinks of phenomenology as important in its own right. I think of it as important as a *Vorwissenschaft* which opens up problems for science.

1. The main purpose of my note was to discuss perception as invariance, because I think that that view of perception gives an operational meaning to the word *of* in the term *perception of*. What you perceive can be thought of as an invariant, and an invariant is always invariant *in spite of* certain specified transformations or changes. To say that you perceive tonal volume is to say that you can plot an invariant for tonal volume, an isophonic contour, under the transformations of phenomenal pitch, phenomenal loudness, stimulus frequency and stimulus intensity. You perceive the size of an object in the Gibsonian visual world, the world of object constancies, when phenomenal size and stimulus-object size are both invariant under the transformation of phenomenal and stimulus-object distance. On the other hand, if phenomenal size and the ratio (linear stimulus-object size)/(stimulus-object distance) are both invariant under the transformations of stimulus size and distance, as happens under the kind of reduction that eliminates clues to distance, then you may be said to be perceiving this invariant ratio or, if you prefer, the retinal size which is, of course, also invariant. In this sense you could be said to have a perception *of* your retinal image, but let no one think that this sophisticated quasi-mathematical conception is Kepler's homunculus in the brain scrutinizing the back of the eyeball. I do not understand Gibson's discussion of invariance but I see nothing in my view that interferes with his phenomenology. I am trying merely to take psychology further along into science.

2. Gibson sticks to his phenomenal dichotomy of the visual field and the visual world. The meaning of his visual world is clear, but I can not understand the nature of his visual field. Since the visual field appears to resemble what the artist draws on paper and paper is flat, it occurred to me that the Gibsonian visual field ought to be the Gibsonian visual world reduced by the elimination of clues to distance. This view introduces difficulties, for such reduction can be partial and not complete, so that there might be a whole series of systems in between the world and the field. Gibson, however, now rejects this hypothesis and I must yield; after all it is his visual field, not mine. Nevertheless I am glad to have

called attention to this variety of perceptual systems that are possible with various degrees and kinds of reduction.

3. Gibson asks us to think that phenomenal size and distance are interrelated and inseparable. Every seen size is at some distance, he says, and its magnitude depends partly on the objective distance and partly on the objective size. If reduction renders objective distance indeterminate as a condition of perception, then attitude takes over and specifies some distance for the size to be at. That view seems to me to be perfectly good for a phenomenology where an object stands up for appraisal by its observer. On the other hand, the view seems to me too naive for final acceptance when one has regarded the problem of stating magnitudes operationally.

Operationally both size and distance, phenomenal or physical, exist only as a relation to a standard of comparison, which may indeed be a tape-measure. Even the expanding universe is expanding only relative to its standard of comparison, the light-year. If light fatigues so that the light-year shrinks, then the universe may not be expanding physically after all. Phenomenal size is just as relativistic as physical size. You sit for an hour watching marionettes perform on their little stage, set up in a large darkened theater. At first the acting figures are small, but soon they get to be no size at all, except as their size is given in relation to one another and to other objects in the scene. Then the show is over, the lights come on, the string-pullers troop out on the stage to take the applause, and the spectators gasp in astonishment, for these men, the string-pullers, are to you and the other spectators live giants, such as you have never seen before. Phenomenal size, like physical size, is relative and has no meaning except as the relation between objects. That is good operational thinking, and I am disposed to think it is good phenomenological thinking too. Does a phenomenal object ever have size until it is related to another object? And is not distance phenomenally indeterminate until you see or imagine something nearer or farther away to make comparison possible?

In any case it seems to me that Gibson is in error in thinking that stimulus-controlled phenomenal size becomes indeterminate when stimulus-controlled distance, under reduction, becomes indeterminate. If seen size got more and more independent of stimulation-size (retinal size) with the increasing elimination of clues to distance, then the m.v.'s of Holway and Boring's determinations of equivalent phenomenal sizes ought, with increasing reduction, to get larger (3). They do not.

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